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THE STABILITY OF BELIEFS[1]

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§ 1 THERE are two ways of holding beliefs. Some are held by the explicit profession of certain articles of faith, as the Apostles' Creed when recited in the words of the *Book of Common Prayer.* The other form of belief is held implicitly by reliance on a particular conceptual framework by which all experience is interpreted.

The process of philosophic and scientific enlightenment has shaken the stability of beliefs held explicitly as articles of faith. To assert any belief uncritically has come to be regarded as an offence against reason. We feel in it the danger of obscurantism and the menace of an arbitrary restriction of free thought. Against these evils of dogmatism we protect ourselves by upholding the principle of doubt which rejects any open affirmation of faith. For the past three centuries the principle of doubt has been continuously at work on the elimination of all uncritical affirmations of faith.

Unfortunately, the protracted application of this cure has had results rather similar to those experienced recently in the therapeutic use of penicillin. The first dramatic successes could not be repeated in subsequent cases, for the diseases to which penicillin was applied have shown a tendency to transform themselves into forms that are resistant to the drug. Indeed, the net result of a continued process of antibiotic and chemical therapy may be merely to breed out a new race of germs completely resistant to all drugs known to man. Similarly, the continued application of doubt seems to have converted all forms of faith into implicit beliefs, ensconced in our conceptual framework, where they elude the edge of our scepticism.

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[1] Paper read to the Philosophy of Science Group on 6 March 1952.

§ 2 Examples of these modern highly doubt-proof beliefs are Marxism and the Freudian doctrine, both of which are especially protected against doubt by the fact that they themselves claim to be embodiments of scepticism. I shall quote two passages to illustrate the adhesive power of these modern interpretative frameworks by which they retain the allegiance of their followers.

> My party education had equipped my mind with such elaborate shock-absorbing buffers and elastic defences that everything seen and heard became automatically transformed to fit a preconceived pattern.[1]
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> The system of theories which Freud has gradually developed is so consistent that when one is once entrenched in them it is difficult to make observations unbiased by his way of thinking.[2]

The first of these statements is by a former Marxian, the second by a former Freudian writer. At the time when they still accepted as valid the conceptual framework of Marx or of Freud—as the case may be—these writers would have regarded the interpretative powers of this framework as evidence of its truth ; only when losing faith in it did they feel that its powers were excessive and specious. We shall see the same difference reappear in our appraisals of the interpretative power of different conceptual systems, as part of our acceptance or rejection of these systems.

§ 3 In this paper I shall try to illustrate the elementary principles by which a conceptual framework retains its hold on the mind of a person believing in it. I shall mention magical beliefs and Soviet doctrines, both of which I reject, and align them with scientific beliefs which I accept. I thus examine the same or similar mental operations in the one case from the outside critically, and in the other from the inside, uncritically.

This is a conscious affront on my part to the critical tradition of modern thought and is bound to shock some readers. It may be proper therefore to define briefly the general grounds on which I stand and mention previous writings in which they are explained.

I hold that the propositions embodied in natural science are not derived by any definite rule from the data of experience, and that they can neither be verified nor falsified by experience according to any definite rule. Discovery, verification and falsification proceed according to certain maxims which cannot be precisely formulated and still less proved or disproved, and the application of which relies in every case on a personal judgment exercised (or accredited) by ourselves. These maxims and the art of interpreting them may be said to constitute the premisses of science, but I prefer to call them our scientific beliefs. These premisses or beliefs are embodied in a tradition, the tradition of science. The continued existence of science is an expression of the fact that there exists a group of people (customarily described as scientists) who are agreed in accepting one tradition, and that they trust each other to be informed by this tradition. But for the continued coherence of scientific opinion which governs scientific life, the meaning of such terms as a ' scientific statement ' or ' a scientist ' would lose most of their present connotations and find their meaning reduced to little or nothing. The whole idiom of science in which its interpretative framework is expressed would lose its character of a living and authoritative language.[1]

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[1] A. Koestler, *The God that Failed,* London, 1950, p. 68

[2] Karin Horney, *New Ways in Psychoanalysis,* London, 1939, p. 7

Holding, as I do, this conception of science and accepting science as true, I must call science a belief which I share. This accreditive expression can be expanded indefinitely by giving my reasons for believing in science and elaborating the nature of this belief; but it can never be exhaustively justified by statements of fact.

I must pass over the epistomological problems which arise at this point, by merely mentioning that, contrary to the traditional usage, I propose to introduce the word ' belief ' in place of the word ' knowledge ', with the intention of keeping always open in our minds a broad and patent access to the personal origins of our convictions. By this conceptual reform I hope eventually to eliminate the difficulties inherent in the various theories of truth, whether they rest on correspondence, coherence or utility. I trust this general statement of my position may induce readers to bear with this discourse a little longer, as I proceed with it now.

§ 4 The several ways in which different people comprehend the world are reflected in the variety of languages in which they express themselves. Of course language manifests a belief only if we use its words with the implied acceptance of their appositeness. People who do not believe in oracles, witchcraft or magic may yet speak of oracles, witchcraft and magic, but they mean *so-called* oracles, witchcraft and magic, putting these words mentally in inverted commas. The use of a language expresses belief in a conceptual framework only if its words are used confidently, without any implied qualifications of unbelief, such as the quotation marks would stand for. The language may then be said to form an idiom of belief.

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[1] Most of these statements are to be found in my *Science, Faith, and Society*, 1946. For their elaboration, see *Logic of Liberty*, 1951 and " Scientific Beliefs ", *Ethics*, 1951.

The fact that primitive people hold distinctive systems of beliefs by practising peculiar modes of interpretation which are inherent in their conceptual framework and are reflected in their language, was first stated with emphasis by Levy-Brühl earlier in this century. The more recent work of Evans-Pritchard on the beliefs of Azande [1] has borne out and has given further precision to this view. The author is struck by the intellectual force shown by the primitive African in upholding his belief against evidence which to the European seems flagrantly to refute them. An instance in point is the Zande belief in the powers of the poison-oracle. The oracle answers questions through the effects on a fowl of a poisonous substance, called *benge*. The oracle-poison is extracted from a creeper gathered in a traditional manner, which is supposed to become effective only after it has been addressed in the words of an appropriate ritual. Azande—we are told—have no formal and coercive doctrine to enforce belief in witch-doctors and their practice of the poison-oracle, but their belief in these is the more firmly held for being embedded in an idiom which interprets all relevant facts in terms of witchcraft and oracular powers. Of this Evans-Pritchard gives various examples.

Suppose that the oracle in answer to a particular question says ' Yes ', and immediately afterwards says ' No ' to the same question. In our eyes this would tend to discredit the oracle altogether, but Zande culture provides a number of ready explanations for such self-contradictions. Evans-Pritchard lists no less than eight secondary elaborations of their beliefs by which Azande will account for the oracle's failure. They may assume that the wrong variety of poison had been gathered or a breach of taboo committed, or that the owners of the forest where the poisonous creeper grows had been angered and revenged themselves by spoiling the poison ; and so on.

Evans-Pritchard describes further the manner in which Azande resist any suggestion that *benge* may be a natural poison. He often asked Azande what would happen if they were to administer oracle-poison (*benge*) to a fowl without delivering an address, or if they were to administer an extra portion of poison to a fowl which has recovered from the usual doses. The Zande—he says—does not know what would happen and is not interested in what would happen ; no one has been fool enough to waste good oracle-poison in making such pointless experiments which only a European could imagine. Indeed, were a European to make a test which in his view proved Zande opinion wrong they would stand amazed at the credulity of the European. If the fowl died they would simply say that it was not good *benge*, the very fact of the fowl dying being proof of this.

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[1] E. E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*, Oxford, 1937

This blindness of Azande to the facts which to us seem decisive is sustained by remarkable ingenuity. 'They reason excellently' (says Evans-Pritchard) 'in the idiom of their beliefs, but they cannot reason outside, or against, their beliefs because they have no other idiom in which to express their thoughts.'

§ 5 With this illustration in mind, we may try to analyse more generally the power of language to embody and firmly to uphold a system of not explicitly asserted beliefs. A language may be defined as a collection of recurrent utterances which carry every time the same or a similar meaning. We may estimate that of the 2000 to 3000 English words in common usage today, each occurs on the average a hundred million times in the daily intercourse of people throughout Britain and the United States. In a library of a million volumes using a vocabulary of 30,000 words, the same words will recur on the average more than a million times. A particular vocabulary of nouns, verbs, adverbs and adjectives thus appears to constitute a definite theory of all subjects that can be talked about. It postulates that these subjects are all constituted of comparatively few recurrent features, to which the nouns, verbs, adverbs and adjectives refer. The theory is somewhat similar to that of chemical compounds. Chemistry alleges that millions of different compounds are composed of a small number—less than a hundred—of always identical chemical elements. Since each element has a name and characteristic symbol attached to it, we can write down the composition of any compound in terms of the elements which it contains. This corresponds to writing down a sentence in terms of a certain language.

So long as we use a certain language, all questions that we can ask will have to be formulated in it and will thereby confirm the theory of the universe which is implied in the vocabulary and structure of the language. It follows that we cannot state without self-contradiction within a language any doubt in respect to the theory implied by the language. The only way to dissent from the theory of the universe implied in a language is to abandon some of its vocabulary and to learn to speak a new language instead. This does in fact happen when primitive people who believe in witchcraft, etc., are gradually converted to the European conception of universal causation.

§ 6 The resistance of an idiom of belief against the impact of adverse evidence, which would impel it to modify its conceptual framework in favour of alternative conceptions, may be regarded under three headings, each of which is illustrated by the manner Azande retain their beliefs in the face of situations which in our view should invalidate them. Some analogous cases can be readily adduced also from other systems of beliefs.

The stability of Zande beliefs is due in the first place to the fact that objections to them can be met one by one. This power of a system of implicit beliefs to defeat valid objections one by one is due to the circularity of such systems. By this I mean that the convincing power possessed by the interpretation of any particular new topic in terms of such a conceptual framework is based on past applications of the same framework to a great number of other topics not now under consideration ; while if any of these other topics were questioned now, their interpretation in its turn would similarly rely for support on the interpretation of all the others. Evans-Pritchard observes this for Zande beliefs in mystical notions. 'The contradiction between experience and one mystical notion is explained by reference to other mystical notions.'

The circularity of the theory of the universe embodied in any particular language is manifested in an elementary fashion by the existence of a dictionary of the language. If you doubt, for example, that a particular English noun, verb, adjective or adverb has any meaning in English, an English dictionary dispels this doubt by a definition using other nouns, verbs, adjectives and adverbs, the meaningfulness of which is not doubted for the moment.

So long as each objection is defeated in its turn, its effect is to strengthen the fundamental convictions against which it was raised. 'Let the reader consider (writes Evans-Pritchard) any argument that would utterly demolish all Zande claims for the power of the oracle. If it were translated in Zande modes of thought it would serve to support their entire structure of belief.' Thus the circularity of a conceptual system tends to reinforce itself by every contact with a fresh topic.

§ 7 Circularity operates by divided roles when a number of persons holding the same set of pre-suppositions mutually confirm each other's false interpretation of experience. Take the following story of a South African explorer, L. Magyar, collected by Lévy-Brühl who regards it as typical.[1] Two African natives, S. and K., go to the wood to gather honey. S. found four big trees full of honey, whilst K. could find only one. K. went home bewailing his ill luck, while S. had been so fortunate. Meanwhile S. had returned to the wood to bring away the honey, was attacked by a lion and torn to pieces.

The relatives of the lion's victim at once went to the soothsayer to discover who was responsible for his death. The soothsayer consults the oracle several times and declares that K., jealous of S.'s rich harvest of honey, assumed the form of a lion in order to avenge himself. The accused denied his guilt strenuously and the chieftain ordered the matter to be settled by the ordeal of poison. 'Matters then followed their usual course'—says the explorer's account—'the ordeal was unfavourable to the accused, he confessed and succumbed to torture. . . . The accusation appears quite natural to the soothsayer who formulates it, the prince who orders the trial by ordeal, the crowd of bystanders and to K. himself who had been transformed into a lion, in fact to everybody except the European who happens to be present.'

It is clear to us that K. had not actually experienced turning into a lion and tearing S. to pieces, and so at first he denied having done so. But he is confronted with an overwhelming case against himself. The interpretative framework which he shares with his accusers does not include the conception of accidental death ; if a man is devoured by a lion there must be some effective reason behind it, such as the envy of a rival. This makes him an obvious suspect and when the oracle, which he has always trusted, confirms the suspicion he can no longer resist the evidence of his guilt and he confesses having turned into a lion and having devoured S. This closes the circle of the argument and confirms the magical framework in which it was conducted, and it thus enhances the powers of this framework for assimilating the next case which will come under its purview.

Communists who have experienced the procedure which leads to confessions in Russian sabotage trials have described a similar circularity. A person who according to Western conceptions is entirely innocent, will confess to acts of sabotage because he shares the fundamental assumption of his accusers that if a boiler explodes, or a train is derailed, somebody has been guilty of sabotage. As in the magical framework, the conception of accidental damage is excluded. The prisoner will usually resist the accusation to start with, but when it is persistently borne in upon him from all sides by the examining magistrate and by the evidence extorted from his former associates, he begins to give way to the convincing power of the case against himself. So he eventually connives in construing against himself any oversight he may have committed—or even any silent discontent he may have harboured—into an act of conspiracy, until he is so covered with guilt in his own eyes that he agrees to atone for it by signing a confession.[1] On the grounds on which he had habitually condemned others he must now condemn himself—and thus close the circle which once more confirms these grounds and makes them stronger than ever for the next occasion.

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[1] Cf. Lévy-Brühl, *The "Soul" of the Primitive*, London, 1928, p. 44

§ 8 To the stabilising power of circularity we may add secondly the capacity of a well developed interpretative framework to supply secondary elaborations to its beliefs which will cover almost any conceivable eventuality, however embarrassing this may appear at first sight. Scientific theories which possess this self-expanding capacity are sometimes described as epicyclical, in allusion to the epicycles that were used in the Ptolomean and Copernican theory to represent planetary motions in terms of uniform circular motions. All major interpretative frameworks have an epicyclical structure which supplies a reserve of subsidiary explanations for difficult situations. The epicyclical character of Zande beliefs was shown above by the ready availability of eight different subsidiary assumptions for explaining a point-blank self-contradiction in two consecutive answers of an oracle. Again we may find analogies in contemporary affairs. The Polish underground leader Stypulkovsky who published his experiences in Soviet Russia recalls that of twelve Poles accused together, he alone refused to confess. This contradiction, however, strengthened rather than weakened the Soviet case : for his denial of the charge was taken to confirm the genuineness of the confessions given by the other accused and thus made the evidence against him stronger than it would have been had he also confessed.

§ 9 Thirdly, the stability of Zande beliefs is seen to rest on the fact that any new alternative conception would have to be built up on a series of supporting facts which can only be adduced one by one. A new conception like that of natural causation would require numerous relevant instances for its proper understanding. But these instances cannot accumulate in the minds of people if each of them is disregarded in its turn for lack of the concept which would lend significance to it.

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[1] Cf. Weissberg, *Conspiracy of Silence*, London, 1952, pp. 128, 202, 318, 352

The behaviour of Azande whom Evans-Pritchard tried to convince that *benge* was a natural poison which owed none of its effectiveness to the incantations customarily accompanying its administration, illustrates the kind of contemptuous indifference with which we normally regard things of which we have no conception. ' We feel neither curiosity nor wonder ' writes William James, ' concerning things so far beyond us that we have no concepts to refer them to or standards by which to measure them.' The Fuegians in Darwin's voyage, he recalls, wondered at the small boats, but paid no attention to the big ship lying at anchor in front of them.[1] This third defence mechanism of implicit beliefs may be called the principle of suppressed nucleation. It is complementary to the principle of circularity. Circularity protects an existing system of beliefs against doubts arising from any adverse piece of evidence, while suppressed nucleation prevents the germination of any alternative concept on the basis of any single new piece of evidence.

§ 10 Circularity, combined with a readily available reserve of epicyclical elaborations and the consequent suppression in the germ of any rival conceptual development, lends a degree of stability to a conceptual framework which we may describe as the measure of its completeness. We may speak of the completeness or comprehensiveness of a language and the system of conceptions reflected by it—as we do in respect to Azande beliefs in witchcraft—without in any way implying approval of the system as a true belief.

§ 11 We do not share the beliefs of Zande in the power of poison-oracles and we reject a great many of their other beliefs, by discarding mystical conceptions and replacing them by a naturalistic explanation. But we may question whether our rejection of Zande superstitions is the outcome of any general principle of doubt. If such a principle exists, it should be possible to detect it in the first place within science which the adherents of the principle of doubt regard as the best example for the operations of this principle.

Every important discovery affects the existing interpretative framework of science in two ways. It strengthens some hitherto accepted scientific beliefs by confirming them, and weakens, modifies or entirely replaces others, by the incorporation of new matter which contradicts or lies outside the score of hitherto accepted conceptions. Thus science may be said to advance by the *assimilation* of fresh topics within its existing system and by the *adaptation* of its existing system to the nature of fresh topics; the first is a conservative act, the second a process of reform. Every scientific discovery is conservative in that it maintains and expands the system of science as a whole, and to this extent confirms the scientific view of the world and strengthens its hold on our minds; but no major discovery can fail also to modify the outlook of science, and some, like those of gravitation or the circulation of the blood, or like those of the genes, quanta, radioactivity, relativity have changed it profoundly.

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[1] *Principles of Psychology*, vol. 2, p. 110. A more recent instance of this occurred when Igor Guzenko, cipher clerk of the Soviet Embassy in Canada, tried in vain for two days in succession (5th and 6th September 1945) to attract someone's attention to the document of the Soviet Atomic Espionage which he was showing round in Ottawa at the risk of his life.

Might it not be true then to say that the assimilative process merely conserves science, while the true innovations consist in the adaptive process by which the framework of science is reformed? This sounds plausible, but it is not true. The power to expand hitherto accepted beliefs far beyond the scope of hitherto explored implications is an eminent force of discovery. It is this force which sent Columbus in search of the Indies across the Atlantic. His genius lay in taking literally and as a guide to practical action that the earth was round, which his contemporaries held vaguely and as a mere matter for speculation. The ideas which Newton elaborated in his *Principia* were widely current in his time; his work did not shock any strong beliefs held by scientists, at any rate in this country. But again his genius was manifested in his power of casting these vaguely held beliefs into a concrete and binding form. As another example we may take the atomic theory of matter which was first introduced into modern science by John Dalton in 1805 for the explanation of the laws of chemical combination. The theory was soon universally accepted; yet 80 years later Van't Hoff met with considerable opposition by deriving from it the asymmetrical arrangement of the four different substituents attached to a carbon atom. Though he had thus merely envisaged in concrete terms what was implicit in notions generally accepted at his time, he was jeered at by the great Kolbe (1877) for having borrowed Pegasus from the stables of the Veterinary Academy at which he was then an instructor. The advent of modern atomic physics, starting with the discovery of the electron by J. J. Thompson, was due to repeated flights of scientific imagination which derived new aspects of Dalton's theory, far beyond its previously apparent perspectives. One of the greatest discoveries of this age, that of the diffraction of X-rays by crystals (in 1912) was conceived by a mathematician, Max von Laue, by the sheer power of believing more concretely than anyone else in the current theories both of crystals and X-rays.

The assimilative power of an existing scientific framework thus appears no less creative and offers no less scope for the application of scientific genius, than its capacity to sprout into new and entirely unexpected forms. Indeed the conservative and the reforming aspects of discovery remain always combined ; we have assimilation to the extent to which new conceptions form an extension of the old and innovation in so far as the new stands in contrast to the old.

What room does such a picture leave for the operation of a principle of doubt ? Is doubt perhaps a true guide to scientists in choosing whether to lean more towards ' assimilation ' or ' innovation ' ? Does not the proverbial scientific caution teach scientists to be harder to convince than are other people ?

The opinion is widespread. But the exercise of special caution is not peculiar to the scientist. The practice of every art must be restrained by its own form of caution. The precision of the lawyer, the poet's fastidiousness, the sculptor's touch, are as many restraints by which these various professions are guided. Naturally, the scientist is also trained to exercise his own manner of restraint, as part of his distinctive art, which is the art of discovery. Caution is commendable in science, but only in so far as it does not hamper the boldness on which all progress in science depends. And there is no rule to tell us at the moment of deciding the next step in research what is truly bold and what merely reckless, and we can therefore have no rule either how to distinguish at such a moment between doubt which will curb recklessness and will qualify as true caution, and doubt which cripples boldness and will stand condemned as unimaginativeness or dogmatism.

We call ' caution ' only that kind of doubt which we consider to be, or to have been, reasonable. Hence ' doubt ' described as ' caution ' acknowledges our appreciation of a successful operation of doubt, without telling us how to achieve such success. We call it true boldness on the part of Einstein that he accepted uncompromisingly Mach's critique of the concept of absolute motion while we praise his caution in rejecting Mach's critique of the reality of atoms. But no principle of doubt could have told him to accept the one and reject the other. ' Caution ' is a form of approval, masquerading as a rule of procedure.

There is one instance on record—by which I may demonstrate this point—of a scientist who tried to apply the principle of doubt in an explicit form. This was the Swedish professor Cleve to whom Svante Arrhenius, then a student, first presented his theory of electrolytic dissociation. Arrhenius has told the story how Cleve said 'This is very interesting', and then 'Goodbye'. Later Cleve explained that there were so many theories formed and these were almost certain to be wrong, for after a short time they disappeared and therefore by reasoning on statistical lines he concluded that Arrhenius' theory would not exist long either.[1]

§ 12 Resistance against the conceptual reform suggested by Arrhenius was widespread and violent among chemists, who thought it absurd to assume that free particles of highly reactive metals like sodium or potassium could float about in water without instantly decomposing it. Yet in a few years' time electrolytic dissociation became so firmly accepted, that its further history offers an excellent example for the extraordinary stability of scientific conceptions in the face of invalidating factual evidence. Arrhenius had postulated a chemical equilibrium between the dissociated and the undissociated forms of an electrolyte in solution. From the very start the measurements showed that this was true only for weak electrolytes like acetic acid, but not for the very prominent group of strong electrolytes, like common salt or sulphuric acid. For more than 30 years the discrepancies were carefully measured and tabulated in textbooks, yet no one thought of calling in question the theory which they so flagrantly contradicted. Scientists were satisfied with speaking of the 'anomalies of strong electrolytes', without doubting for a moment that their behaviour was in fact governed by the law that they completely failed to obey. I can still remember my own amazement when, about 1919, I first heard the idea mooted that the anomalies were to be regarded as a refutation of the laws postulated by Arrhenius and to be explained by a different theory. Not until this alternative conception (based on the mutual electrostatic interaction of the ions) was successfully elaborated in detail, was the previous theory generally abandoned.

Contradictions to current scientific conceptions are often disposed of by calling them 'anomalies'. This is among the most handy assumptions in the epicyclical reserve that is available for the adaptation of any theory, in the face of adverse evidence. We have seen how Azande make use of similar adaptations to meet the inconsistencies of poison-oracles. In science this process has often proved justified by subsequent re-interpretations of the original theory which explained the anomalies.

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[1] E. N. da C. Andrade, *Encyc. Brit.* 14th ed., article on Arrhenius.

§ 13 Another example may illustrate the reverse case, namely when a series of observations which at one time were held to be important scientific facts, were a few years later completely discredited and committed to oblivion, without ever having been disproved or indeed newly tested, simply because the conceptual framework of science had meanwhile so altered that the facts no longer appeared credible.

Towards the end of the last century numerous observations were reported by H. B. Baker [1] on the power of intensive drying to stop some normally extremely rapid chemical reactions and to reduce the rate of evaporation of a number of commonly used chemicals. Baker went on publishing further instances of this drying effect for more than thirty years.[2] A large number of allegedly allied phenomena were reported from Holland by Smits [3] and some very striking demonstrations of it came from Germany.[4]

H. B. Baker could render his samples unreactive sometimes only by drying them for periods up to 3 years; so when some authors failed to reproduce his results it was reasonable to assume that they had not achieved the same degree of desiccation. Consequently, there was little doubt at the time that the observed effects of intensive drying were true and that they reflected a fundamental feature of all chemical change.

Today these experiments, which aroused so much interest from 1900 to 1930, are almost forgotten. Text-books of chemistry which thoughtlessly go on compiling published data still record Baker's observations in detail, merely adding that their validity 'is not yet certainly established' [5] or that 'some [of his] findings are disputed by later workers, but the technique is difficult '.[1] But active scientists no longer take any interest in these phenomena, for in view of their present understanding of chemical processes they are convinced that most of them must have been spurious, and that, if some were real, they were likely to have been due to trivial causes. This being so, our attitude towards these experiments is now similar to that of Azande towards Evans-Pritchard's suggestion of trying out the effects of oracle-poison without an accompanying incantation. We shrug our shoulders and refuse to waste our time on such obviously fruitless enquiries. The process of selecting facts for our attention is the same in science as among Azande, but I believe that science is more often right in its application of it.

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[1] *Journal of the Chemical Society of London*, 1894, **65**, 611

[2] Cf. *ibid.*, 1922, **121**, 568; 1928, Pt. I., 1051

[3] Smits, *The Theory of Allotropy* (1922). Baker's experiments are referred to (p. vii) as 'the most beautiful means of establishing the complexity of unary phases' postulated by the author.

[4] Coehn and Tramm, *Ber. deutsch. Chem. Ges.* 1923, **56**, 458; *Zeitsch. f. Phys. Chem.* 1923, **105**, 356; 1924, **110**, 110; and Coehn and Jung, *Ber. deutsch. Chem. Ges.* 1923, **56**, 695. These authors reported the stopping of the photochemical combination of hydrogen and chlorine by intense drying.

[5] F. A. Philbrick, *Textbook of Theoretical and Inorganic Chemistry*, revised edition, London, 1949, p. 215

§ 14 This last sentence may sound curious and yet it is meant to give you a serious sample of what I believe to be the correct way of reflecting on the difference between Zande magic and the teachings of science. Science and magic are both comprehensive systems of beliefs, possessing a considerable degree of stability, and a comparison of the two systems has shown that the convincing powers of both are derived from similar logical properties of their conceptual frameworks. Yet the two achievements of stability are not on par, but are mutually exclusive. If you accept one system you cannot hold the other, and we today overwhelmingly accept science. The critical movement of the last 300 years has tried to sanction the acceptance of science while avoiding any explicit declaration of faith, which was contrary to its basic programme. I believe that this attempt has failed because it is logically mistaken, and that, consequently, it can never succeed at all. I hold that we have good reasons for preferring science to magic or astrology, or (what is of greater practical importance) to the perversion of science imposed by Stalinism on the territories under Communist rule. But I suggest that these reasons can never be adequately stated without a personal affirmation of belief on the part of the speaker.

We may recognise this by reflecting on the foregoing analysis of the stability of beliefs. No extension of this preliminary sketch into further detail could break through its logical limitations : it would remain a study of other people's beliefs. It might describe in further detail the way beliefs are upheld in the face of occurrences which, to a person not holding these beliefs, would appear to contradict them, and perhaps it might explore also the historic and social antecedents which led to the establishment of certain beliefs within a particular society. But however far we pursued the study of other people's beliefs this would never indicate whether we held these beliefs to be true or illusory and, if so, on what grounds we decided this.

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[1] J. R. Partington, *General and Inorganic Chemistry*, 1946, p. 483. Thorpe's *Dictionary of Applied Chemistry*, Article " Benzene and its Homologues " (1947) reports Baker's ' interesting discovery ' without any qualification.

§ 15 At any rate, there would be no possibility of indicating this if we refrained from smuggling into our description of the way beliefs are held, expressions which imply affirmations of our own beliefs. The confusion of the two is sanctioned by common usage. Phrases like ' it is commonly agreed ' or ' it will hardly be doubted ' are literally statements about what people are supposed to believe or not expected to doubt, yet they are commonly understood as declaring the certainty of the beliefs in question. Or take words like ' science ' and ' scientist '. A neutral analysis of science as a system of beliefs should always use the word ' science ' and ' scientist ' in quotation marks, in the way the Soviet Academy refers to bourgeois ' science ' and bourgeois ' scientists '. The confident use of any expressions including the word ' science ', or its derivatives like ' scientific method ', ' scientific observation ', ' natural law ', etc., convey the writer's belief in a certain body of allegations, in the rightness of a certain procedure for arriving at such allegations and of confirming them, without his ever having taken the responsibility for affirming this belief. The use of the scientific idiom by writers on scientific method establishes in fact from the start a tacit understanding between them and their readers on the trustworthiness of the method which they are setting out to analyse.

§ 16 Writers on the nature of science who unquestioningly believe in science and may assume the same of their readers, will find no difficulty in carrying out an analysis of science in objective terms. They may define science as the simplest description of the facts or the most economical survey of sense data ; they may pretend that science is not concerned with the truth or that it only makes provisional statements so as to provide stimulus for new experiments. They may say that science is a free creation of the mind, forming part of a conventional game, or that its value lies entirely in its usefulness. As long as everybody is tacitly agreed about the nature of science and implicitly accepts the authority of science, it may not become apparent that statements of this kind only refer to certain formal aspects of science which do not account for its authority. I suppose there should be no difficulty for a positivistically inclined member of the Zande tribe to describe the system of magic accepted by Azande as the simplest description of the facts, or as the most economical survey of sense data, or as a conventional framework valued for its usefulness.

§ 17 The situation is different once a system of beliefs is fundamentally challenged. It must then be defended on its true grounds. I suggest that for this purpose our beliefs, including our belief in science, will have to be declared explicitly, in fiduciary terms.

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ARE THERE QUANTUM JUMPS?

PART II*

E. Schrödinger

7 *The Cultural Background, Again*

It might be maintained that the case I am making in this essay is quite irrelevant. It might be said that the 'privilege' is not meant seriously, that I am naïvely taking it at its face value. We may, so my opponent might continue, use the pure energy states of *any* system ; just take our choice so that they are convenient to handle analytically. Any state of any system may be regarded as some superposition of some or all of its proper modes (= pure energy states). One may deal with them separately, *as if* the system was in one of them ; the several results, duly superposed, will then tell us what would come of their superposition, if we really want to know. Perhaps my supposed opponent would even grant that he is using just a convenient shorthand, the same as in chemistry and statistical thermodynamics ; but he would maintain that this is perfectly admissible also for investigating a single event of interaction between micro-systems.

To this I have two answers, referring to two very different points. The first is this. Even if this shorthand were admissible for the micro-event, we have to keep in mind that physicists are not the only people interested in physics and in the outcome of theoretical research in physics. Those results that are tenable and will survive are destined, eventually, to be absorbed into our world picture and to become part and parcel thereof. A great many of our educated contemporaries, not equipped with the mathematical apparatus to follow our more technical deliveries, are yet deeply concerned with many general questions ; one of the most stirring among them certainly is whether actually *natura facit saltus* or no. Whatever abbreviated language we physicists may find convenient to use among ourselves, we ought to be aware of the dilemmas that justly and duly interest others ; we must be careful not to veil or distort them by indulging in loose speech. It is this point that I had in mind in my general historical introduction.

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* Part I appeared in volume III, no. 10, of this *Journal.*

Science is not a soliloquy. It gains value only within its cultural milieu, only by having contact with all those who are now, and who in future will be, engaged in promoting spiritual culture and knowledge. The extant scientific papers of Archimedes, the dialogues and discourses of Galileo, are still of genuine interest in our day, and not only to philologists, but to many scientists. Would it mean setting ourselves too high and too proud a goal, if we occasionally thought of what will have become of our scientific papers 2,000 years hence? Science will have changed entirely. Will there be anybody to grasp our meaning, as we grasp the meaning of Archimedes?

8 *Details about Collisions*

This was the first part of my answer to the supposed opponent. The second part is more technical, and ought to be supported by a little mathematics which I must avoid here. I doubt whether in dealing with a micro-event (' collision problem ') one is allowed to pick one's ' initial state ' arbitrarily as composed of pure energy states of the two non-interacting systems, relying on the principle of untainted superposition. In point of fact, this procedure regularly leads to very acceptable results in the first meaningful approximation and to entirely unacceptable ones in the next step of approximation. Now it so happens that in the wave equation the term that depicts the interaction (engendered by that relatively small third part of the energy operator which we spoke of in Section 5), is, of necessity, a *non-linear* term; it must contain the *product of the two wave-functions*, otherwise it could not entail any interaction between them. Thus by the very task it has to fulfil this term must infringe upon linearity, and thereby do away with clean and simple superposition. And it is, of course, from this term, and from this term alone, that the results are deduced. Now general mathematical considerations tell us in a case of this kind, three things, namely

(i) starting from any initial situation, the first small change caused by the perturbating term is correctly computed from the latter in spite of its non-linearity; but

(ii) the changes computed in this way for two or more initial situations are not simply additive, when an initial situation, composed of the former, is contemplated; there is a substantial mutual influence of the components; moreover

(iii) to improve on the first approximation is a very intricate task, if the perturbing term is not linear ; the current routine follows, as far as I can see, the pattern introduced for *linear* perturbations. Thus it is insufficient to cover the actual situation.

There may, of course, be other reasons for the actual failure of the approximation method, a failure now usually referred to as 'the divergences.' We have loosely called the interaction operator 'relatively small.' Now this epithet cannot properly apply to an operator, only to the quantity which results from applying the operator to another quantity (in our case : to the wave-functions). This result is different in different parts of the field. There may be regions where the 'relative smallness' does not hold. It may be justifiable to disregard this fact, and the approximation method may none the less yield acceptable results. Even so one must be prepared to discover a fundamental shortcoming. This is well illustrated by the first application of this method of approximation, leading to the earliest quantitative achievement of wave mechanics, viz. my computation, in 1926, of the splitting of the spectral lines of atomic hydrogen by an external electric field (Stark effect). Both the frequencies and the intensities agreed very satisfactorily with observation. Yet a few years later Cornel Lanczos discovered a fundamental shortcoming in my statements. Compared with the field of the nucleus, the perturbing external field is weak only in the neighbourhood of the nucleus, where the internal field is strong ; the ratio is reversed at moderate distances, since the internal field fades away rapidly according to the inverse square law while the external field is constant. This has the consequence that none of the Stark effect lines is really sharp, an effect that is readily observed in the higher members of the spectral series when a fairly strong external field is applied.

There is another point that I consider relevant in collision problems. Towards the end of Section 6 I mentioned that two micro-systems which vibrate, each of them truly and exactly in one of its proper modes, cannot be conceived as approaching each other. This is not a far-fetched subtlety. Translatory motion with precisely fixed velocity is wave-mechanically represented by a plane sinusoidal wave filling the whole space. Two such plane waves, one pertaining to the first, the other to the second system, do not exhibit any feature representing 'distance from each other.' They describe a state that virtually includes all possible distances down to the smallest at which the interaction already is 'in full swing.' To choose for the *initial state* one that includes these two plane waves seems hazardous. One thereby disregards the building up of the interaction during the gradual approach. I am more surprised at the acceptable results this procedure yields in the first instance than at its ensuing failure. Sometimes it is permissible to think of one system as localised. One would then at least have to picture the other as a plane wave *with a wave head* or wave front, approaching the point where the collision is to take place. It is known from pre-quantum physics that what happens at a wave front may differ widely from the stationary state that establishes itself by and by in its rear. For example, it was shown by A. Sommerfeld that when a plane electromagnetic wave passes from vacuum into a dielectric (pictured as composed of molecules that contain electronic oscillators), the head of the wave is not refracted according to Snell's law, but continues indefinitely in the same direction with unchanged velocity of propagation. The reason is that the electrons are initially at rest; each of them is seized upon by the oncoming wave train and is gradually rocked into oscillations of increasing amplitude until an equilibrium state is reached after many waves have passed. True, this model is obsolete. But the means used now to depict the physical situation still follow the pattern to which it belonged. I do not believe that the intricacies involved in the 'classical' problems have passed away. I find it hard to believe that quantum physics holds the master key for overcoming these intricacies without attacking them. In the above example, taken from Lorentzian electronics, the initial state, which shows us an undisturbed incident wave and an electron at rest, differs very substantially from the final state, when the electron is in full swing and superposes its wavelet on the incident wave. (Indeed, all these wavelets together turn the original plane wave into one of entirely different velocity and entirely different direction.) In Lorentzian electronics this gradual transition cannot possibly be dealt with as a problem of 'perturbation.' One cannot hope that in quantum physics this method will yield an exhaustive answer, unless one maintains that according to quantum physics nothing of the sort happens at all, and that the whole analytical apparatus is set up only for telling us with what probability the system may choose to hop from one state into another, these 'states' being patently selected so as to meet our analytical convenience and ability. Is that not wishful thinking?

9 *On Neo-Machian Purism*

There is, of course, among physicists a widely popular tenet, informed by the philosophy of Ernst Mach, to the effect that the only task of experimental science is to give definite *prescriptions* for successfully foretelling the results of any future observations from the known results of previous observations. If this contention is taken at its face value, then *not only* is it irrelevant whether the prescription makes use of a visualisable model or only consists in definitely prescribed mathematical operations, to be performed with the previously observed values in order to obtain predicted values ; *but also* the said operations would apparently not even have to be mathematically correct, as long as they are precise and enable us to prophesy correctly. Indeed the lack of rigour in a theoretical deduction is nowadays sometimes followed by the remark that what vindicates the procedure is its success, its leading to results that agree with observation. The neo-Machian principle covers this argument not only pending the proof that the theoretical deduction is correct, *but even if it should prove to be wrong.*

Nobody will agree. But why? If our task is only to predict precisely and correctly by any means whatsoever, why not by false mathematics ? Because, as little as in any other science, does the scientific method in physics consist of a vast number of juxtaposed independent prescriptions for the purpose of prophecy. Whether or no they are based on a visualisable model-picture, those prescriptions, if one wants to give them this abstract name, form an intricately connected and interwoven scheme. Even in the simplest quantitative experiment half a dozen of them must be relied upon for applying corrections to the rough numbers read on the scales, before you obtain what you call the result of your measurement. Moreover, there are basic statements or assumptions, from which a thousand branches of inference issue, which coalescing in bundles with many others eventually lead off and on to a relevant new ‘ prediction.’ And in every single experiment, though it be not of a new type, this network of inferences has to be produced afresh, adapted to the special data and circumstances. The ‘ scheme of prescriptions ’ is not satisfactory unless we trust it to embrace, suitably applied, *any* future observation. We have no other means for rendering tolerably precise the application to new cases that were not foreseen, nay for drawing any conclusions at all with respect to them, but to decide once and for all to use the self-consistent scheme of correct mathematics. Mathematics is not a very special thing. It is the most general way of contemplating the possible relationships between numbers without contradiction. A single 'formula,' provided it contains at least one undetermined symbol, (and does not exhibit an explicit zero as a denominator), cannot be 'mathematically wrong.' Those 'prescriptions' of physical science contain at least two undetermined symbols, viz. at least one representing a previous observation, and one that is to be predicted. So no single prescription can be mathematically wrong. But we have never to do with one alone. We have to combine them in rational thought. Without a guide we should be groping in the dark. Correct mathematics is the indispensable guide even to the hardiest Machian purist. Most physicists, whether or no they confess to it, are using some kind of model-picture besides.

So much for vindicating my clamour for mathematical consistency in handling collision problems.

10 *Numbers*

The states of sharp energy (whose privilege of being *the* states of a system, with abrupt transitions between them, I contest) include the characteristic that the total number of particles of every kind contained in the system be an integer. According to Einstein's famous discovery (1905) that mass and energy are the same thing, a particle of rest-mass m, when at rest, represents an amount of energy mc^2 (c = velocity of light in vacuo). Broadly speaking, n equal particles contained in a system contribute the amount nmc^2 to any of the sharp energy levels of the whole system, which differ therefore on this account by integral multiples of mc^2. This is an entirely sound, nay, an unavoidable assumption, even though we have not yet succeeded in accounting satisfactorily for the various numerical values of m, actually observed in the various elementary particles, as electrons, protons, the various mesons, etc., from which the masses of atoms and molecules are composed.

From the point of view taken here we must consider it inadequate to think, e.g. a body of nitrogen gas in front of us as consisting of a definite integral number of nitrogen molecules that could be indicated. It has to be represented by a superposition of proper modes with a considerable spread over many integers. We should wish to estimate the extension of this spread. This, it must be granted, is not so easy.

Though it is *not* a question of fluctuations, one naturally thinks that considerations of the kind used in thermodynamics might come to our rescue. However, the thermal energy of one degree of freedom ($\frac{1}{2}kT$) is—except perhaps at the temperature T of the interior of a star —so small compared with the rest energies mc^2, that in ordinary circumstances no system containing ponderable mass must be considered in *true* thermodynamic equilibrium.

The following attempt to answer the question suggests itself. Our isolated body of nitrogen gas has the same properties as one that forms part of a much larger body from which it is only severed by fictitious boundaries ; indeed our body might have been obtained from a much larger one, the fictitious boundaries being replaced by real ones. It is universally agreed that the number N of molecules contained within a volume delimited only in thought exhibits fluctuations of the order of $\sqrt{N}$. I consider it reasonable to assume that the wave function of the isolated system would in general be composed of eigenfunctions whose N-values spread over a region of order of magnitude $\sqrt{N}$.

It may be asked : what difference does this make ? Could it be tested by experiment ?—The main difference is that it is reasonable, while it is unreasonable to admit a sharp count N of objects that avowedly lack individuality.[1] Secondly, this is the obvious way of registering the fact, that we *never* experiment with just *one* electron or atom or (small) molecule. In thought-experiments we sometimes assume that we do ; this invariably entails ridiculous consequences as, e.g. that a spherical de Broglie wave, which is supposed to represent 'one' electron, moving in an 'unknown' direction, suddenly collapses into a small wave parcel, when 'that' electron is detected at a definite spot. Nothing of the sort happens if the number 'one' is not ascertained, but may as well be zero or two or three. Even better : the certain detection of one does not reduce the expectation of a second or third, it leaves it unchanged, according to generally accepted principles of statistics. And, I do not regret to say, we sorely need those spherical waves as realities (not merely as expressing our lack of knowledge), if we wish to account, e.g. for G. P. Thomson's beautiful experiments on the interference patterns of de Broglie waves, diffracted by crystals ; and the same in many, many other cases.

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[1] The lack of individuality of micro-systems is argued in my article in *Endeavour,* July 1950, **9**, No. 35 ; reprinted in the Smithsonian Repoıt for 1950, pp. 183-196 ; in German, *Die Pyramide,* January and February, 1951.

11 *Observing Single Particles*

In writing this I have before me the lovely records of single particle tracks, of 'stars' (nuclear disintegrations), of 'showers,' of broken tracks showing successive decays of one particle ($\pi \rightarrow \mu \rightarrow e$), the photographic emulsion records that C. F. Powell selected to illustrate his Nobel lecture on cosmic radiation in 1950. I am asking myself whether here as well as in the tracks of particles produced in the Wilson cloud chamber we are not actually experimenting with a quite definite number of particles, in some cases really with just one particle, and whether we are not presented with records of quantum transitions, which I am so anxious to deny.

In the first place it is fair to state that we are not *experimenting* with single particles, any more than we can raise Ichthyosauria in the zoo. We are scrutinising records of events long after they have happened. That the interval of time is shorter and that we have ourselves produced favourable conditions for the records to produce themselves, makes no difference. We can never reproduce the same single-particle-event under planned varied conditions; and this is the typical procedure of the experimenter. But now to the other question: are these not records of quantum transitions?

They certainly do *not* show us the transformation of a couple of plane waves (representing the colliding particles before the collision) into a number of other plane waves (representing the particles that emerge after the collision). Here my opponent interrupts me: 'Well, that's just the point. That is why your waves must not be regarded themselves as a real observable phenomenon, but only as indicating the probability of particle-phenomena.' I mentioned just before that there are many experiments which we simply cannot account for without taking the wave to be a wave, acting simultaneously throughout the region over which it spreads, not 'perhaps here' or 'perhaps there,' as the probability view would have it. I shall come back to this point.

The real difficulty for the wave aspect is the exact balance of energy (and momentum) *in the single event*, for which the said records (in the photographic plate and in the Wilson cloud chamber) are full evidence. Take the simplest case: an α-particle of given initial velocity in the cloud-chamber. It has a well defined range; it is always stopped after travelling a distance of, say, 5 cms., and after producing a certain number of ionisations, which number is practically always the same and corresponds to the particle spending its kinetic energy and coming to rest. How are we to understand this, if ionisation is to mean *not* the transfer of an energy parcel, but only a resonance phenomenon ? Well, there would be no difficulty in understanding it, if the slowing down were due to a static gravitational or electric field against which the particle runs up. In this case, if we picture the particle as a composite wave-phenomenon (usually called a 'wave-parcel'), this would, according to the theory, actually be slowed down as a whole and come to rest as a whole, always at the same distance, when its 'kinetic energy is spent.' What happens is that all the constituents have their wave-length slowly, 'adiabatically,' increased so that the group keeps together. To meet the actual case we are speaking of one has to adopt the view that the ionisable medium influences the particle that passes through it in the same adiabatic fashion as a field would. I cannot see that this view is quite unacceptable once we have shaken off the nightmare that physical events consist in continual sequences of little fits and jerks, the handing over of energy parcels from one particle (or group of particles) to another.

I believe that not even the apparently catastrophic events oblige us to accept the 'fit and jerk view'; I mean the events recorded as 'stars' showing a nucleus shelled to pieces by the impact of a swift particle. They only occur with *exceedingly* fast particles of *exceedingly* high wave frequency. This encourages one to think that they too may be slow, adiabatic processes ; for the criterion of 'slowness' always is : slow compared with the rapidly oscillating wave-field. One has, of course, to use the auxiliary concept, familiar to quantum physicists, of wave parcels in more than three dimensions, actually three times the number of particles that come into play. To enlarge on this here in general terms would have little value. Besides I am quite aware that eventually there remains the momentous question : what are these apparent particles anyhow ? Can it be understood that they turn up within continuous wave trains—somewhat like the white crests in a choppy sea—and that in some cases they constitute the only observable features of these wave trains ?

I am aware of these questions. They are no longer as embarrassing as they were, before we had gained the insight we have now gained into what a particle certainly is *not ;* it is *not* a durable little thing with individuality.[1] However, let me conceive of an opponent to

[1] See my article in *Endeavour*, July 1950, 9, No. 35.

whom the preceding paragraphs of this Section II seem lame excuses and loose talk. To him I should still plead as follows.

The cloud chamber and emulsion phenomena, though they are at the moment in the focus of interest, represent after all only a small section of all that we know about nature. In their apparent simplicity they appeal to the vivid imagination of an intelligent child. They would have set any of the old warriors for the cause of atomism, from Democritus down to Dalton and Boltzmann, gasping with joyful excitement. Yet they are not as simple as they look. This is witnessed by the pages and pages of intricate formalism that is often devoted to account for even the simplest of them. We must not be so rash as to remove the obvious difficulties presented by these new and unprecedented observations, by remodelling our picture of the physical world in a manner that makes it unfit to give us an understanding of a great many other things that are at the moment not fashionable.

To give an example : if one scans the introductory chapters of Max von Laue's standard works on the diffraction of X-rays [1] and on the diffraction of material waves,[2] one finds that no pattern of thought has yet been discovered to get on in these matters even one step without regarding the wave functions—Maxwell's field and the de Broglie amplitudes of both the incident wave and those it encounters in the diffracting body—as describing something real. Here ' real ' is not a controversial philosophical term. It means that the wave acts simultaneously throughout the whole region it covers, not either here or there. That would fail to account for the interference phenomena. So the epithet ' real ' means the momentous difference between ' both-and ' (*et-et*) and ' either-or ' (*aut-aut*). I challenge anyone to dispute this discrepancy away, if he can. I do not think there is any escape from this : if you accept the current probability view (*aut-aut*) in quantum mechanics, the single-event observation becomes comparatively easy to tackle, but all the rest of physics (unfashionable at the moment) is lost to sight.

[1] *Roentgenstrahlinterferenzen*, Berlin, Akad. Verl. Ges., 1941

[2] *Materiewellen und ihre Interferenzen*, ibid. 1948

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THE ELECTRIC CURRENT. A STUDY OF THE ROLE OF TIME IN ELECTRON PHYSICS*

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'The word current is so expressive in common language that when applied in the consideration of electrical phenomena we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it.'[1]

Faraday was right ; a century later electron theory is still not wholly free from that prejudice. *Into which electrical processes does time enter in the manner suggested by the idea of a current?* The answer is not certain. The present paper therefore examines the status of time in electron physics.

We shall be able to view the history of electrical theories with a sharper critical sense if we first observe that two fundamentally distinct methods are available, at least in principle, for representing electrical phenomena. Though one of these has not yet been developed, the mere recognition of its possibility enables us better to appreciate the significance of the other.

Electrical systems are essentially *polar*, an electric field being represented by a polar vector, and the task of an electrical theory can be regarded as the representation of polar vectors, or states of polarisation, and their changes. (Magnetic effects and axial vectors are here neglected.) In the present state of physical thought two contrasted methods can be conceived for representing changing states of polarisation :

(A) *As the relative (linear) displacement of entities of opposed signs (charges) which themselves persist unchanged*, for example as the movement or propagation of two opposed types of material fluids, of permanent particles, or of persisting disturbances in a medium. In the theories using this method electric charges are assumed to be permanent, the two opposed types of charge are treated as fundamental concepts and polarity as a consequence of the relative displacement, in the line of the polarity, of pre-existing charges of

* Received 18. iv. 52

[1] M. Faraday, *Experimental Researches in Electricity*, 1849, § 1617

opposed sign. This *dynamical* method is illustrated in Lorentz's electron theory. Differences between opposite charges (for example, electron/proton mass) and the creation and vanishing of pairs of charges lie beyond its scope. The limitations of this method can be seen more clearly when it is contrasted with the alternative, in which changing states of polarisation are represented :

(B) *As deformations of a polar character in a structural arrangement of neutral entities*, for example as a polar distortion of the equilibrium pattern of a system composed of neutral particles. (As a simple model of a polar deformation one can imagine a transformation of the angular coordinates of any spherical arrangement,[1] such as a shell of stationary nucleons or electrons, producing a contraction of the shell near one pole and an expansion near the opposite pole.) Here the structural arrangement, with its changing polar distortion, is treated as fundamental ; polar properties, including the apparent presence of opposite charges, are consequences of a polar deformation, and the appearance or disappearance of pairs of charges may result from changes in the degree of polarisation. Current densities and apparent velocities of electrical particles are interpreted as rates of change of system polarisation, not involving the displacement of charged particles. This *structural* method is potentially more powerful than (A) in relation to non-constant charges, but it has not yet been made the basis of a mathematical theory. (We shall see that Schrödinger's theory can be regarded as a transition from theories of type (A) towards a possible future theory of type (B).)

How does time enter these two methods ? (A) involves motion from a point 1 at t_1 to a point 2 at t_2 where the transit time[2] $(t_2 - t_1)$ is positive and measurable, so that the process is characterised by a finite particle or group velocity and the energy is localisable as the kinetic energy of a dynamical entity. In (B) there are no point-events of departure and arrival, there is no action which passes from one part of the system to another and takes time in doing so, and there is no linear velocity of displacement of any entity. Instead there is a structural (? angular) change in the system and all its parts, involving an instantaneous correlation of parameters throughout a finite system. There is no *time of transit* but there may be phase relations and a system *relaxation time* or *oscillation period.* The energy is not localisable, being the potential energy of a strain in an extended structure.

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[1] On spherical point arrangements see my forthcoming paper in *Am. Math. Monthly*, 1952.

[2] The term *transit time* is used here for a measurable time interval between the moments of departure and arrival of some entity such as a particle or a wave-packet.

The contrast may be made more precise by considering the fact that an element of a steady current can *for certain purposes* be regarded either as the displacement of a charge through a distance, or as a change of electric polarisation P.[1] The principle of the conservation of charge, expressed as a differential equation, then has the following equivalent forms :[2]

$$\frac{\partial}{\partial t}\cdot\rho + \operatorname{div}\,(\rho\,.\,v) = 0 \qquad \text{A}^\star$$

$$\frac{\partial}{\partial t}\cdot(\operatorname{div}\,\mathrm{P}) + \operatorname{div}\left(-\frac{\partial \mathrm{P}}{\partial t}\right) = 0 \qquad \text{B}^\star$$

where (div. P) corresponds to the volume density of charge ρ, and the change occurring in a small volume per unit time is represented in A* by ρ times v, a velocity, and in B* by the rate of change of polarisation $\left(-\frac{\partial \mathrm{P}}{\partial t}\right)$. A* displays conservation in terms of the flow of a fluid of variable density, and B* as an algebraic identity based on the interpretation of surface charge as the condition on the opposite faces of a polarised structured medium. Thus both account naturally for the experimental fact of conservation.

But while A* exhausts the relevant content of the method (A), B* is an incomplete formulation of (B) and should be supplemented by an expression representing the polarisation P as a function of the spatial structure of the system. This has not yet been done in accordance with (B). All existing structural theories of polarised systems (dielectrics, molecules, atoms, etc.) assume the existence of charges and interpret the polarisability of such systems as due to the relative displacement of the charges. But (B) requires a theory which displays polarisation and surface charges as properties of structures composed of neutral entities, subjected to deformations of a polar character.

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[1] Both methods appear to fail in the case of steady super-conducting circuits, where a steady current appears to correspond to a constant state of polarisation rather than to a rate of change of polarisation.

[2] Cf. E. T. Whittaker, *History of the Theories of Aether and Electricity*, vol. 1, Edinburgh, 1951, p. 175

A full treatment of the relation of (A) to (B) must determine whether one is capable of including the other as a special limiting case, or whether the two represent distinct terms which must both be included in a complete expression of the electric current. But here we are only concerned with the time aspect.

Since (A) involves a transit time and (B) does not, which electrical processes, if any, are characterised by a transit time? Have all charged particles the same status in this respect?

Using this empirical question to sharpen the analysis we can now turn to the history of the concept of a current.

The early period from Gilbert to Faraday was occupied in accounting for the connection between electrical effects at different points in space, and for the conservation of certain electrical properties through time. It was natural, in view of the background of thought at the time, that this was accomplished by treating all electrical phenomena as the spatial displacement of electric fluids or particles. This idea became precise in Nollet's conception (1749) of two electrical 'streams' in the discharge of a Leyden jar, which led directly to Galvani's and Volta's interpretation of their discoveries in terms of an electric 'current'. Thus all the theories developed were of type (A). In 1813 Berzelius considered whether the electricities and caloric were matter or merely phenomena (for example some disturbance in an aether regarded as non-material), but in both cases linear motion or propagation was assumed. The idea of a structural theory of electrical phenomena does not seem to have occurred to anyone during this period.

Faraday was perhaps the first to recognise the possibility of (B), though he rejected it as regards conduction in wires. He sought to give definite expression to

> . . . what is called the voltaic current, in contra-distinction to any supposed peculiar state or arrangement, not progressive, which the wire or the electricity within it may be supposed to assume. . . . Now if the (voltaic) troughs merely act by causing a peculiar arrangement in the wire either of its particles or its electricity, that arrangement constituting its electrical and magnetic state . . . [then some deflection of the magnetic needle should occur before the circuit is closed.] But if the magnetic effects depend on a current, then it is evident why they could not be produced in *any* degree before the circuit was complete ; because prior to that no current could exist.

> By current, I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or, speaking still more generally, progressive forces. By *arrangement*, I understand a local adjustment of particles, or fluids, or forces, not progressive. Many other reasons might be urged in support of the view of a *current* rather than an *arrangement*, but I am anxious to avoid stating unnecessarily what will occur to others at the moment.[1]

These passages, written in 1832, display an admirable scientific caution and clarity of analysis, if one considers how little attention had been paid to structural arrangements at that time. Yet Faraday's argument is no longer compelling. For, to take one consideration alone, it is now well known that all the effects of an electric current (other than local heat production) can appear even when the circuit is not complete in the sense of solid contact, for example in radiation fields or in discharges *in vacuo*. Faraday recognised the danger of the facile term 'current', and yet, at least as regards conduction in wires, he gave his authority to the prejudice which it created.

Clerk Maxwell also considered the possibility that a current in a wire might consist of a structural rearrangement, and followed Faraday in rejecting it :

> The electric current cannot be conceived except as a kinetic phenomenon. Even Faraday, who constantly endeavoured to emancipate his mind from the influence of these suggestions which the words 'electric current' and 'electric fluid' are too apt to carry with them, speaks of the electric current as 'something progressive, not a mere arrangement'.
>
> The effects of the current, such as electrolysis, and the transfer of electrification from one body to another, are all progressive actions which require time for their accomplishment, and are therefore of the nature of motions.[2]

But 'the transfer of electrification from one body to another' is *not* necessarily a spatially progressive action, involving earlier effects at one point and later ones at another. For example, this is so in the movement of ions in electrolysis, but not in the discharge of a condenser, where no such causal asymmetry can be discerned. Moreover, many progressive actions, for example the passage of a pulse along a nerve fibre, have not the character of 'motions', if this is taken to mean the linear displacement of persisting entities. Indeed Maxwell's own work contains an important example of the representation of electrical phenomena otherwise than as kinetic or propagated actions, for he used an instantaneous non-propagated potential, involving a correlation of simultaneous parameters throughout an extended system of the type assumed in (B). But Maxwell's instantaneous potentials were dropped as having no physical significance in the case of radiation, and hitherto no application has been found for them.

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[1] M. Faraday, *Experimental Researches in Electricity*, 2nd ed., 1849, vol. 1, §§ 282, 3

[2] J. C. Maxwell, *Treatise on Electricity and Magnetism*, 3rd ed., 1892, vol. 2, p. 211

Maxwell produced many ideas which have since been justified. But one may still be awaiting exploitation. He was convinced that it was necessary to provide a structural interpretation of the displacement current.[1] It is true that he conceived this in mechanical terms and sought to account for the polarisation of the aether as a local phenomenon, in terms of minute spheres and vortices. Moreover, from the present point of view his term 'displacement' was doubly ill-chosen : it seems to imply (i) that the electric polarisation of a medium results from a mere spatial separation of pre-existing electric charges ; and (ii) that the changes occurring are linear shifts, whereas they may be angular deformations of an equilibrium pattern. Yet Maxwell's idea may still prove right. Properly understood the concept of deformable structure may underlie that of polar and axial fields ; polarised structures may be the source of all polar phenomena.

Throughout the nineteenth century the dynamical interpretation dominated, and nearly all the physicists of the time followed Faraday and Maxwell in regarding *all* types of electric current as kinetic phenomena. But this assumption was not universally accepted. For example, in 1888 O. Heaviside [2] was led by the consideration of non-steady currents in wires to express scepticism : 'I have been forced to make Maxwell's scheme intelligible to myself, to go further and add that the electricity may be standing still, which is as much as to say that there is no current, in a literal sense, inside a conductor.'

With the development of electron theory and the measurement of the electron charge and mass the assumptions underlying (A) appeared to receive decisive confirmation. Evidence accumulated showing, as it seemed, that conduction in gases, in electrolytes, and in solid conductors was all of the same fundamental character and consisted in the transfer of permanent elementary units of electric charge. For example, Thomson's determination of the velocity of cathode rays, by deflection in combined electric and magnetic fields, was one of many experiments supporting the idea of electrical particles undergoing motions in three-dimensional space.

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[1] A discussion of the manner in which time enters Maxwell's displacement current lies outside the scope of this paper, which is primarily concerned with electrons.

[2] O. Heaviside, *Electrical Papers*, vol. 2, p. 394

But in none of the determinations of electron velocity has the transit time of an electron, or of a single pulse of electrons, been directly measured, e.g. *against a mechanical clock or an electromagnetic pulse.*

Three types of method have been used for measuring electron velocity :

I By direct comparison of the transit time against a mechanical clock. In 1894 J. J. Thomson[1] attempted to measure the velocity of cathode rays against a rotating mirror, but the experiment must be regarded as unsuccessful, and no other application of this method has been made.

II By deflection in constant electromagnetic fields. No time period is measured. Spatial curvatures are determined, and v/c (the ratio of the velocity of the electron to that of light) is inferred from them.

III By balancing the transit time against the cycle of high frequency oscillatory fields used to deflect or arrest an electron beam.[2]

Thus the only experiments which have any claim to be regarded as successful measurements of a transit time are those using method III. If these are interpreted in terms of the classical particle picture, there is no reason to doubt that they provide a determination of a true velocity. Moreover, the results obtained are consistent with many less direct determinations of v, for example by method II. But this apparently satisfactory position becomes less clear when viewed in terms of Quantum Mechanics.

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[1] J. J. Thomson, *Phil. Mag.*, 1894, **38**, 358. The time measured was too large, and G. P. Thomson, in a private communication, suggests that it may have been due to a time lag resulting from unsteady conditions.

[2] The Wiechert-Kirchner method. F. Kirchner, *Ann. d. Physik.*, 1932, **12**, 503 ; C. T. Perry and E. L. Chaffee, *Phys. Rev.*, 1930, **36**, 904. All transit time effects of electron tubes with varying fields may be regarded as falling under this head (see D. Gabor, ' Dynamics of Electron Beams,' *Proc. I.R.E.*, Nov. 1945). See also F. G. Dunnington, *Phys. Rev.*, 1933, **43**, 404.

The particle interpretation of electronic processes has been drastically modified by Wave Mechanics, the uncertainty and complementarity principles, the principle of the uncontrollability of the transfer of a quantum, and the statistical theory of Quantum Mechanics. It is replaced by an abstract, higher dimensional, statistical calculus representing *stationary states*, in which the probability density is independent of the time or nearly so, and the relation of this calculus to observations of the movement of particles of small mass under changing fields is obscure. The Heisenberg inexactitude principle does not prevent the determination of 'electron velocity' with any desired accuracy.[1] But under Quantum Mechanics 'electron velocity' can only be retained as a statistical concept connecting the probabilities of observing an electron (or a group of electrons) at particular points in space-time under particular experimental conditions in an ensemble of similar experiments. A true 'velocity', that is a parameter representing the time derivative of the space coordinate of one entity moving in a given direction, or the ratio of a finite distance travelled to time taken, can only be derived from a probability distribution in higher space when that distribution is equivalent to *a disturbance of sufficiently constant form in 3-space displaced at a nearly constant rate.*[2]

Such constancy of the three-dimensional form of a probability function can only occur in the absence of excessive fields and interactions. For example, even in the simplest case of a monochromatic electron wave of exact wave length and momentum, the presence of a finite field or interaction results, *inter alia*, in some partial reflection of the wave and introduces an uncertainty, not merely in the magnitude, but *in the sense or direction of the phase velocity*, this uncertainty of direction of phase propagation being a basic characteristic of wave mechanics.[3] Moreover, the wave mechanical representation of electronic phenomena in constant fields, like geometrical optics, does not involve a measurable time parameter. Thus the classical conception of velocity as the time derivative of the space coordinate of a persisting entity cannot be taken over into the quantum mechanics of electrons, photons, mesons, etc., though it may provide an adequate first approximation under exceptional conditions where only sufficiently weak and constant fields are present.

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[1] W. Heisenberg, *The Physical Principles of the Quantum Theory*, Chicago, 1930, p. 25; E. C. Kemble, *Fundamental Principles of Quantum Mechanics*, New York, 1937, p. 60.

[2] i.e. a steady wave-packet in 3-space with a group velocity not subject to rapid change.

[3] Cf. J. Fraenkel, *Wave Mechanics. Advanced General Theory*, Oxford, 1934, Chap. i.

But if this interpretation is correct, what is the significance of the time interval which has been measured in the determinations of 'electron velocity' using method III?

If the wave-packet representation is considered satisfactory, this time interval can be regarded as a true transit time, since in these experiments the deflecting fields were too weak and their rate of change too small to produce excessive reflection or dispersion of the packet. *But the presence of a transit time cannot be positively inferred from experiments using periodic fields and many pulses.* Moreover, there remains some doubt regarding the applicability of the wave-packet method to an experiment in which there is a finite rate of transformation of energy, from electrical energy to heat. This method provides no theory of the creation of the wave-packet or of its conversion into heat energy, that is, does not account for the initial or terminal conditions of the process. And under more general conditions (fields either stronger or changing more rapidly) the particle concept fails, and only the probability function remains.

This analysis serves to expose the crucial theoretical and empirical question: Is it *necessary* to assume a time interval $(t_2 - t_1)$ associated with the departure and arrival of an electron or a single pulse of electrons?

On the theoretical side it is conceivable that the determinations of v using method III may be open to an interpretation using a phase velocity but not involving a directly measurable transit time for single electrons, or possibly even for groups of electrons.[1] Most electronic phenomena are better pictured as stationary wave fields,[2] or as pulsations or oscillations in a stationary frame than as the propagation or displacement in one direction of waves or particles. Thus apparent transit times may be replaceable by system oscillation or relaxation periods, and the electron tube, magnetron, etc. may have to be regarded as complex types of cavity resonator, similar to that used in recent determinations of the velocity of light.

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[1] For example, the null deflection might depend on a relation between the cycle of the oscillatory EM fields and a vibration period of the electron field, no one-way linear propagation of a wave-group being assumed.

[2] But stationary states and standing waves are idealised limiting cases which do not actually occur. All physical phenomena involve a finite rate of transformation of energy, and interactions and decay rates must be included in the basic expressions of a complete theory.

But experiment must decide. Can the transit time of a single electron or pulse be measured without using periodic electromagnetic fields or other interactions which may affect the course of the process? Or, more generally, for which wave-particles can the transit time of a single pulse be directly measured? No theoretical analysis can provide a substitute for experiments establishing the precise realm in which the concept of particle or group velocity is indispensable. It was already pointed out in 1927 [1] that 'this hypothesis of electron velocities is one which has never yet received direct experimental confirmation. . . . It therefore becomes very important for the experimental physicist to discover whether he can measure the distance travelled in a measured fraction of a second', that is without using periodic effects or interactions which may introduce complicating effects.

The dissolution of the simple dynamical picture was carried a stage further by Dirac's relativistic theory of the electron. This resulted in a separation of (observable linear) *velocity* and (quantum mechanical) *momentum* and involved an entirely different relation between these two quantities from those either of classical or of relativistic (non-quantised) mechanics. Moreover, in Dirac's theory the instantaneous ratio of distance to time is not equal to v, and always has the eigenvalues $\pm c$. This paradox was mathematically resolved by Schrödinger, who showed that a tremor or oscillatory motion of high frequency is imposed on the instantaneous velocity c such that the average velocity over a finite time is equal to the observed 'v'. But this tremor is not directly observable and the awkward divorce of momentum and velocity remains. It seems that the unsolved problems of relativistic quantum theory may be linked with some erroneous assumption in the basic idea of a *moving electron*.

In a restricted context the particle and wave representations can be treated as complementary and of equal status. But on a wider view this interpretation is misleading. Particle theories must start with the coordinates of the particles, and this is inadequate if transient particles are to be covered.[2] A complete system of particles, including some which appear and disappear, is better represented by means of a comprehensive wave field. Thus the wave field concept is potentially more powerful than the particle concept, and can be regarded as a transition between theories based on (A) and a future theory of type (B) :

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[1] L. L. Whyte, *Archimedes or the Future of Physics*, London, 1927, pp. 41-42

[2] See W. Heisenberg, 'Paradoxien des Zeitbegriffs in der Theorie der Elementarteilchen', *Göttingen Festschrift*, Göttingen, 1951

(A) Point particles	Q.M. of Wave Fields Extended fields	(B) Extended system deformations and pulsations
Localised kinetic energy	Non-localisable energy	Non-localisable energy
Linear velocity	Particle velocity concept dissolving	No group velocity
		? Angular changes
Charges constant	—	Charges appear and disappear

This survey reveals a curious situation : theoretical advances have progressively dissolved a conception which experiment has so far apparently confirmed. This is a warning against reliance on general theoretical speculation, and an invitation to further experiment. We can no longer evade the question : Under what conditions, by what methods, and in what kinds of electrical processes is a particle transit time directly measurable? This appears to be the most important outstanding empirical problem in fundamental physics.[1]

Heisenberg [2] has shown that the difficulties of basic theory can be traced to the role of time in the processes of elementary particles, and has considered how they might be overcome by modifications of quantum theory. Dirac [3] has sought to make a fresh start by revising classical theory. But the next step may lie in the laboratory.

It is beyond reasonable doubt that the concepts of a transit time and of a directly determinable velocity are indispensable in relation to nucleons and systems composed of them. Nucleons, and the larger systems which they form, are at least sufficiently stable and possess adequate individuality to permit a satisfactory classical interpretation of the many experiments in which their velocities have been determined. There is no ground to question the ordinary interpretation of the experiments in which the velocities of neutrons, protons, or atomic rays have been measured, for example using gravitation or inertial motion in a rotating system. But the lighter 'material' particles (electrons, positrons, mesons) display a much less stable individuality. Neither the theory of Quantum Mechanics nor any observations yet made render the concepts of a transit time and a group velocity absolutely indispensable in representing their fields.

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[1] For example, this is prior to the issues raised by D. Bohm (*Phys. Rev.*, 1952, 85, 166), who is concerned with a breakdown of the accepted interpretation only at very small distances.

[2] W. Heisenberg, *loc. cit.*

[3] P. A. M. Dirac, *Proc. Roy. Soc.*, 1951, A. **209**, 291

Thus the fundamental particles may fall into two classes which are distinguished not merely by a quantitative parameter (mass), but a deeper-lying theoretical difference. This possibility, which has already been considered,[1] receives support from a consideration of the contrast between proton and electron. In any future theory which explains their mass ratio in terms of underlying geometrical and temporal properties, this ratio must appear as a relation between quantities which, though dimensionally equivalent, are geometrically contrasted, as for instance are the circumference and diameter of a circle. It is appropriate to search for a clue to this contrast by examining the manner in which time enters the processes undergone by protons and electrons. Protons may truly move, i.e. display a directly measurable particle velocity in 3-space as assumed in (A), while electron wave fields may be phenomena requiring a representation of type (B). The source both of the mass ratio and of the time paradoxes in the theory of elementary particles may lie in the fact that beneath the dimensional equivalence of the corresponding parameters of particles of different masses may lie two contrasted kinds of process, distinguished by different spatial and temporal characteristics. A magnitude possessing the dimensions L/T may be a true linear velocity in one case, and a normalised rate of change of polarisation (for example, $\left.\frac{1}{\text{div. P}} \cdot \frac{\partial \text{P}}{\partial t}\right)$ in the other. Only experiments which have not yet been made can decide if this hypothesis is correct.

The crucial test would be an attempt to determine electron velocity without using periodic fields, for example by comparing the transit time of a single pulse against some form of mechanical clock. For example a high speed rotor with slits might be used.

Various results are conceivable. Here we shall only consider two. If a transit time were obtained confirming the relation $\frac{1}{2}mv^2 = e \, . \, \text{V}$

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[1] E.g. W. Heisenberg, *loc. cit.*

for low v, it would then be desirable to vary the conditions by using different electron densities and introducing stationary and oscillatory fields, in order to determine the laws governing the breakdown of the concept of a transit time. Only such experiments can establish the true relation of the classical concept of electron velocity to the quantum mechanical concept of the probability of particular observations in an ensemble of similar experiments.

On the other hand, if the observed v did not correspond to the expected value, and the transit time appeared to be too small to measure, that is if v appeared to be independent of the potential and to be indefinitely close to c, it would be permissible to assume that $v = c$, for all values of V. It would then be appropriate to use assumptions of type (B) in the search for a new interpretation of the significance of the 'v' and 'v'$/c$ determined by the earlier experiments and used generally in electron theory. This result may perhaps be regarded as no more unlikely than the Michelson-Morley demonstration of the constancy of c was at the time when that experiment was first proposed. In any case it is wise to take nothing for granted.

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Angles in Fundamental Physics

It has been suggested that the future theory of particles involves a primary field whose mathematico-physical character has still to be identified.[1] Advance towards this may require greater attention to angles. Linear[2] metrics are fully developed, while the consequences of treating angles as the basic geometrical invariant have not been explored. To gain insight into the constants characterising particles and their interactions it may be necessary to substitute angles for lengths in primary expressions. Arbitrary constants may be due to using linear coordinates.

1 A feature of physics since 1900 is the disappearance of similar geometrical figures of different size and the presence of natural units of length, such as e^2/mc^2, h/mc, $h^2/4\pi^2me^2$, etc. This suggests representing physical lengths by a geometrical magnitude also possessing a natural unit: angle. Since only ratios of length are observed, single lengths can be dropped and ratios of variable lengths to natural lengths represented by angles. For example, if l_0 is a natural unit of length a variable length r can be replaced by $\theta = \tan^{-1}\left(\frac{l_0}{r}\right)$ with $0 < \theta < \pi/2$, or $\theta' = \sin^{-1}\left(\frac{l_0}{r}\right)$. Thus it is in principle possible to discard linear metrics, the quantitative property ascribed to single lengths in linear geometrics being replaceable by angles corresponding to ratios of lengths in a geometry based on angles. (This is like the use of a non-homogeneous linear metric; the geometry of the angles about a point in 3-space is isomorphic with that of lengths in plane elliptic geometry.) Just as the relativity of uniform velocity led, together with the natural velocity c, to the use of 4-vectors, so the relativity of lengths, with the natural lengths, suggests the use of angles

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[1] W. Heisenberg, *Two Lectures*, 1949; L. L. Whyte, this *Journal*, 1951, 1, 303.

[2] 'Linear' is throughout used in the geometrical sense.

Pending development of this idea, one can seek to transform concepts involving single lengths (which are redundant and may contain wrong implications), or pairs of lengths, into concepts based on angles. Thus :

(a) *The universal length associated with all nucleons*, say (or the finite size of particles), does not necessarily imply the need for a minimum length or a granular geometry, since this conception can be replaced by the simpler *postulate of the angular equivalence of all nucleons.* For similar systems of nucleons must then be indistinguishable, and therefore possess the property now expressed as ' of the same scale'. The existence of one fundamental length is implicit in this postulate.

(b) ' *The fine-structure constant α is a ratio of fundamental lengths* '[1] *transforms into* ' *α represents an angle with special structural properties not yet identified.*' Beneath the dimensional equivalence of any pair of constants (e.g. c and e^2/h) may lie a geometrical distinction and an angle responsible for their ratio.

(c) Expressions like *mean curvature of space*, *finite* (*or expanding*) *universe*, etc. are ambiguous unless a standard is specified, and invite transformation.

2 Atomism is very naturally expressed in a geometry of angles. For it implies that physics is not concerned with abstract continua, but with the definite figures, observable in 3-space, formed by particles, that is with the geometry of the systems of angles made by the lines from one particle to others. Properties ascribed to ultimate particles should be interpreted as angular properties of these changing figures. In the macro-theory of two-body systems a linear metric was appropriate and angles did not appear explicitly in the laws, but in the micro-theory of many-particle systems this is reversed. In atomic processes angular relations (momentum conditions, scattering correlations, etc.) are primary and linear relations relatively trivial.

3 The properties derivable from Pauli's Exclusion Principle and the corresponding nuclear rule express a principle of closure alien to theories of linear arrangements, which cannot ascribe special properties to systems composed of particular numbers of particles. But in the theory of spherical (angular) point systems[2] arrangements of

[1] L. L. Whyte, *Nature*, 1950, **166**, 824. Also *loc. cit.* n. 1

[2] L. L. Whyte, *Am. Math. Monthly*, 1952, **59**. Appearing simultaneously

particular numbers of points are discriminated as possessing unique extremal properties.

4 In contemporary physical theory wide use is being made of pure numbers: constants (universal numbers; ratios of masses, lives, etc.; ratios of material constants) and variables (dynamical variables in natural units; quantum numbers; ratios of longitudinal to transverse magnitudes; phases; coherence ratios; quality factors; hydrodynamic and thermodynamic numbers, etc.). One can regard this system of numbers as the expression in linear co-ordinate theory of principles in the angular geometry of systems of particles and *every unexplained pure number as the sign of a neglected structural angle.* Angles present in divergent waves, particle interactions, polarised systems, and steric relations generally, may have been missed, either empirically owing to their smallness or theoretically owing to their non-dimensional character. For example, electric polarisation may sometimes arise, not from the linear displacement of existing charges, but from an angular deformation of a system of neutral particles.[1]

Thus *atomic principles may find their most powerful expression in terms of angles.* Cartesian and polar coordinates are interchangeable, but the laws of the physical discontinuum may require angular formulation. The geometry of point systems reveals no parallel between the regular linear arrangements (for example the 65 infinite regular linear point systems) and the extremal angular arrangements (for example the unique spherical point systems); [2] each demands its own calculus. In the study of unique angular point distributions and their transformations both pure mathematics and the mathematical physics of atomism enter a relatively unexplored field.

L. L. Whyte

[1] L. L. Whyte, this *Journal*, 1952, **3**, 243–255.
[2] See ref. n. 2, p. 257.

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The Dictum Of Descartes

I HAVE read Professor Ashby's reply[1] to my previous letter with great interest, but am still unconvinced by his argument. Particularly, I am not at all satisfied that the information theory he quotes is correctly applied in those of his examples where the 'information output' is greater than the 'input'.

(1) In the biological example (p. 50), surely it is not the rules of natural selection which constitute the 'information input', but *the initial complexity of the species concerned* (not only its physical complexity at any one time but some totality of complexity which includes its ability to evolve according to the rules).

As Professor Ashby so rightly says, 'the complexity that can be generated by evolution is independent of the complexity of these rules'. It depends in no way on their information content but only on the information content of the species itself. Indeed, 'the rules' are purely empirical inventions of observers, such as Darwin, to explain their observations; they must be dependent on the design of the species rather than that design being dependent on them.

Looked at in this way, I still believe Descartes' dictum to be true.

(2) Again, in the case of teaching a child (p. 56) the 'initial information' is surely in the design of the child (in its ability to learn) and not in whatever its father teaches it, important though that may be. The child may certainly become more perfect than its father, but I suggest it can never become more perfect than its 'designer'.

(3) The examples of the magnetic sorting machine and the Geiger counting machine are rather more complicated, for at first sight they really do appear capable of contradicting Descartes. However, I think the difficulty here is due to neglecting the information which each machine *must* contain about *every* new item (ball or particle) which may be fed to it for analysis. In the first case this information might be that *all* balls are either magnetic or non-magnetic (a partly-magnetic ball would not be identified as such); in the second case it might be that *all* particles have, say, energies within a certain range of values.

Such information is evidently of a 'latent' nature, for it apparently becomes greater in quantity with each successive operation of the machine; yet it *must* all have been included in the initial design or the machine could not have functioned.

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[1] W. Ross Ashby, this *Journal*, 1952, **3**, 44–57

There is perhaps an analogy in the selection of cards from a pack (Professor Ashby's example on pages 45-46). Here the amount of information to be obtained (or of uncertainty to be removed) by drawing a card could not have been assessed without prior information about the nature of *all* the cards in the pack. The difficulty does not seem to lie in the information theory itself, but in its application to particular circumstances.

(4) Finally, Professor Ashby's specification (p. 52): 'Take a planet with some carbon and oxygen; irradiate it with sunshine and cosmic rays; and leave it alone for a few hundred million years' would hardly be effective in producing a bird unless (I admit this might occur) a great many other conditions were fortuitously fulfilled. The specification is in fact incomplete, and I suggest that to make it complete it would be necessary for the specifier (the designer) to have 'at least as much reality and perfection' as the bird he wished to produce.

B. M. ADKINS

SOCIAL SCIENCE [1]

Mr Collier's book is intended to provide the 'inquiring adult' with a rapid survey of the sciences of man and society. It is written clearly, in a jolly style (if the reader 'does not like mathematics he can skip the lot!'). The author's attempt to make everything painless results in occasional banalities, e.g. 'If parents continually let a small child have its own way, especially when he makes a fuss at being denied, they give a sanction to his self-will.'; 'The most frequent complaint of wives is of their husbands' selfishness, and of husbands that their wives nag.' The list of problems with which Mr Collier flirts is impressive: the nature of science (whose distinguishing mark is said to be the *certainty* of its laws which are *established* by the evidence), the mind-body problem, free-will, heredity, statistics, neurology, Pavlov, endoctrinology, intelligence-testing, Freud, anthropology, values, telepathy, social surveys, starvation, unemployment, vocational guidance, world-order, etc. Penguin-books figure prominently among the references.

In view of its purpose the book has two main defects. The first is its disconnectedness. Thus a few early remarks on heredity are never referred to later, even *à propos* of the problem of natural and acquired dispositions, while the two pages on the brain are quite divorced from the rest of the book. Not all Mr Collier's numerous parcels of information are consistent with one another. On p. 52 the concept of instinct is rejected as hopelessly vague, yet on p. 113 the reader is introduced to something called 'instinctual value'—'goals presented by instincts are seen as good-in-themselves.'

The blurb claims that Mr Collier will 'put the reader at once into the *feel* of scientific thinking.' But the book's most serious defect is that it achieves the reverse of this. The author makes everything seem too easy—even to the extent of blandly ignoring the real problem. This is particularly marked in a section attractively decorated with graphs and diagrams and misentitled, 'The Causes of Slumps.' Any schoolboy can explain why initial recovery from a depression tends to promote further recovery. The question is: What originally caused demand to stop falling and start rising? Keynes devoted a book mainly to this question, but Mr Collier merely says, 'What causes this is uncertain,' and hurries on to the more congenial task of explaining what happens afterwards. He does try to explain what halts a boom—in terms of what actually has the opposite effect, namely, an increase in capital investment! (He thinks 'Saving' and 'Investment' mean the same, whereas the whole point of Keynes' trade cycle theory is that an increase in the propensity to save is deflationary, and in the propensity to invest, inflationary.) And all this under the heading, 'Scientific Method in Economics'!

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[1] *The Science of Humanity*, K. G. Collier, Thomas Nelson and Sons Ltd., Edinburgh, 1950. Pp. xx + 333. 12s. 6d. *Testament for Social Science*: An Essay in the Application of Scientific Method to Human Problems, Barbara Wootton, George Allen & Unwin Ltd., London, 1950. Pp. vii + 197. 15s.

However, Mr Collier's claims for science are modest and reasonable, and he always warns the reader when his own remarks go beyond the bounds of science.

So much cannot be said for Professor Wootton's book, the sub-title of which is a misnomer: she does not herself apply any scientific method to any problem, human or otherwise (a serious defect, since that is 'the only method by which we ever learn anything,' p. 183). About one fifth of her pages are *on* scientific method, the rest being largely devoted to attacks on Christianity, with animadversions on astrology, the press, and party politics, and some insensitive remarks on Art. A chapter on 'Science and Morals' will be considered later.

The author takes it for granted, without argument, that society is 'the field of our most disastrous failures,' a field in which man is pitifully incompetent (p. 1). (Yet she belongs to a society which has practically abolished starvation, violence, and political and legal corruption, and which peacefully replaces its governments.) She attributes this alleged disastrous incompetence to insufficient social science. On the other hand, 'rigorous devotion to scientific method' has resulted in man's mastery over his material environment—in steam-engines, etc. (p. 1). (Was James Watt a devotee of scientific method?) Scientific method is 'the tool' which, if applied fast enough (p. 4) and if accompanied by 'a revolution in the mental attitude of the public' (p. 5), may just enable us to escape wholesale disaster. The recipe is simple: observe facts, invent hypotheses, and verify them.

When philosophers of science talk about the 'hypothetico-deductive method' they are not, of course, referring to a recipe for discovering laws, but to a logical feature of scientific systems: namely, that the entailment of observation-statements by universal scientific propositions is not reciprocal. Consequently, observation cannot *verify* these universal propositions, which must necessarily remain hypothetical.

All this has escaped Mrs Wootton, who believes that 'scientific method' is an organon, or heuristic procedure, and that a hypothesis which has been checked against the facts is thereupon promoted to the rank of a natural law whose validity is certain, conclusively proved, or 'finally established' (pp. 94, 128, 176).

She accepts the myth that in science everything ultimately depends on pure sense-data, and she warns against the ' temptation to be careless about distinguishing primary perception from secondary inference ' (p. 12). But what she means by ' primary perception ' is obscure : apparently, if you walk into a wall in the dark, your sense-data will consist (not of pain and shock, but) of the belief, ' This is a wall ' (p. 7). Either Mrs Wootton has not heeded her own warning, or else she has a peculiar idea of a ' primary,' ' brute ', or ' primitive ' sense-datum. Moreover, sense-data themselves require ' accurate observation ' (p. 10), which would presumably involve further sense-data which would need further accurate observation. . . .

Anyway, these ambiguous sense-data are the basis of both the natural and the social sciences (p. 8). And since only physical occurrences are observable, Mrs Wootton plunges for physicalism : ' even such comments on a union meeting as that it was " orderly " or " peaceful " are fundamentally statements about its physical properties ' (p. 8). She does not ask why the commentator supposes that he is observing trade union members rather than, say, talkative people sheltering from the rain. The answer is, of course, that he tacitly imputes to them certain fundamentally non-physical properties, i.e. intentions, expectations, information, status, etc., in the light of which he can interpret their behaviour.

Although Mrs Wootton rejects the ideas of cause and effect, they colour her notion of a scientific law which, she says, asserts (not that A *causes* B but) that A is *associated* with B, either always, with a certain probability (p. 16). Consequently, she does not realise that one law may connect an infinite range of initial conditions with their respective consequences.

Despite her physicalism, she considers that all social sciences should be regarded as branches of psychology (p. 24). Psychology does, of course, play an important part in the social sciences, but to insist that they belong to ' a sub-division of the social branch of psychology ' is to ignore the fact that they study, *inter alia*, institutions which unexpectedly fulfil roles for which they were not designed, and that economics, for instance, mostly deduces the surprising and unintended regularities which result from individuals' ordinary economic behaviour. (On this Mrs Wootton merely comments that ' economists were inclined to go crazy about deduction,' p. 26.) She does not appear to realise that while psychology studies human behaviour, the social sciences are largely concerned with the unintended consequences of that behaviour.

After some remarks on controlled experiments and the errancy of human behaviour, she urges social scientists to define more terms, to construct more technical vocabularies, and to record and measure more facts. With these familiar nostrums she concludes her chapter on method. In a later chapter she criticises, first, the use of biological analogies in sociology ; and secondly, ' the pseudo-scientific system associated with the name of Marx.' Professor

Popper devoted a book and a series of articles [1] to the thesis that prophecies of the kind made by Marx are methodologically indefensible. Mrs Wootton does not consider this possibility, although Popper's views have been widely discussed (which makes her claim that her method yields progressive additions to knowledge ring rather hollow). She merely contends that part of Marx's prophecy was inaccurate, while 'on the remainder of the prophecy, it is perhaps prudent to suspend judgement for a while' (p. 85). She detects no philosophic weakness in Marxism; why, then, is it a 'pseudo-scientific system'? Mrs Wootton bases her answer on a consideration of personal attitudes; it is because Marxists are dogmatic and superstitious (p. 87).

Professor Hayek used the word 'scientism' to depict an approach to social problems which was in fact thoroughly dogmatic and unscientific although claiming to apply the methods of science. The remainder of Mrs Wootton's book is an essay in scientism. Wrongly supposing that scientific successes are the result of following the 'observation-hypothesis-verification' recipe (she disapproves of the view that an element of unregulated genius is involved, p. 15), and ignoring the essential feature of scientific practice, namely, its open, interpersonal, critical character, she condemns parliamentary discussion and judicial procedure as being based on 'the assumption that two misrepresentations add up to one true representation' (p. 57). The value of traditional experience is not denied because it is not considered: e.g. 'parliamentary procedure would be quickly and radically transformed if it was judged as a scientist judges efficiency' (p. 57); 'the work of the cultural anthropologists is the beginning of wisdom in international affairs' (p. 24). Aesthetic criticism, which 'tends to be descriptive and even subjective' should be refined until it becomes 'precisely quantitative': emotions experienced by creators and spectators of works of art should be correlated with the physical changes in their brains, and the physical properties of works of art should be measured exactly (pp. 166-167).

Leaving aside her warnings about the extraordinary danger (p. 132) of believing 'such an astonishing story' as Christianity, we come to her chapter on 'Science and Morals.' Science, she says, can prescribe means but not ends. However, the frontier between means and ends can be pushed back until we have only one, ultimate, end. When its nature has been established, the remaining territory may be cultivated by scientific method.

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[1] K. R. Popper, *The Open Society and Its Enemies*, London, 1945; 'The Poverty of Historicism,' *Economica*, 1944-45. In 'A survey of work in the philosophy of history, 1946-1950,' Jonathan Cohen writes: 'I have seen no attempt in 1946-50 to refute Popper's arguments concerning the error of what he calls "historicism" (i.e. the claim to understand the laws of historical development).' *The Philosophical Quarterly*, **2**, 7, 1952, p. 182, n. 50.

Moreover, while science cannot uniquely determine this ultimate end, there are ' hints to be derived from science about the nature of moral values ' (p. 136), especially from biology, which suggests to us that ' survival is good,' and that the end of the human organism is to ' persist in its own being and reach its normal completion or actualisation ' (p. 149), or to attain ' mental and physical health.' This, Mrs Wootton holds, is the ultimate moral end—' neither the pig satisfied nor Socrates dissatisfied : but Socrates satisfied.' (She does not mention his presumably immoral decision to drink the hemlock.)

If this appears to be suggested by biology it is only because Mrs Wootton draws no distinction between a scientific description of an end which human beings actually seek, and a moral prescription of an end they ought to seek. Draw that distinction and her attempt to found morality on science collapses. If biology says they do seek this end it is superfluous for the moralist to urge them to do so ; and if biology does not say that they seek this end the moralist has no scientific justification for urging them to do so. The position might be rescued by saying that it is the duty of art to complete what nature is unable to bring to a finish ; then we should be back with Aristotle, of whose views Mrs Wootton has offered an unwitting trivialisation. (I say unwitting because she would presumably not have offered it as something indicated by recent scientific findings if she had known that it had all been said much better 2,300 years ago.)

For a concluding comment we may, I think, borrow from Mrs Wootton herself : ' one of the most tragic features of our present plight is the enormous wastage of ineffective or misdirected goodwill.'

J. W. N. Watkins

BEHOLD, MY CHILD, THE NORDIC DOG

To Lorenz animals are what words, numbers and mountains are to other exceptional people. In few disciplines of zoology can such people exploit the sensibility of observation which their passion gives them. They owe Lorenz a debt for creating a study of animal psychology in which they can be at home.

In this book [1] he writes about twenty-five species, mainly fish, birds and mammals. Many others are mentioned.

The two best essays are on his jackdaw colony, and his vivarium for water shrews. These are two extremes of culture methods.

The jackdaw colony he established on the roof of his home, intending that the animals should behave undisturbed by captivity. To attempt this is typical of Lorenz' desires and methodology. He gives details of how he psychologically attached them to his house. How, not being in the company of adult birds, they were not afraid of human beings, and had to be protected from predators. How he discovered what they innately fear, how they learnt by the experience—*not of danger*—but of a spectacle, which in normal surroundings would almost always coincide with danger, but which in an environment of human artefacts they encountered without danger. How they communicate their memory of this experience, so that in an established colony the young are warned of the dangers of the countryside. He tells of their social hierarchy, and how clearly they can be seen to know one another as individuals ; of the way this hierarchy protects the weakest ; how rapidly any alteration in it is acknowledged by every bird. A male jackdaw after a betrothal of a year marries for life a female below him in the peck order. From her betrothal onwards she is raised to her husband's rank, as in our own species. He makes clear that to speak of the betrothal and marriage of some animals is no more anthropomorphic than to speak of their head or mother. The English naturalists who first spoke of a bird's *mate* used that word to emphasise this. It has become debased comparatively recently. He describes how the old look after the young of the whole colony, both in common and exceptional difficulties ; how he lost birds for years and how they returned causing revealing readjustments of peck order. And he tells a lot about his own mind in the process.

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[1] *King Solomon's Ring*. New Light on Animal Ways, Konrad Z. Lorenz, Methuen & Co. Ltd., London, 1952. Pp. xix + 202. 15s.

The rearing of water shrews from an artificial weaning is a tour de force, and Lorenz falls into the Cellini-like conceit of a great technician. The shrews were in a closed container whose description raises many questions of animal keeping. Should we aim at what looks to us like a natural habitat ; should we frequently clean it, or leave it to oscillate round an ecological equilibrium in which we play a subsidiary part, though the concentration of parasites and decomposing excreta is much larger than in any normal environment ? How much do a species' sensory perceptions and motor reactions differ from our own so that animals must escape, or injure themselves to show us the danger ? No one who reads this book will ever believe again that keeping an animal decently means providing it with a temperature at which it will survive, and substances which it will eat, or even acknowledging it may have light preferences. Providing an animal with a single fellow prisoner of the opposite sex is often the cruellest thing that can be done to one member of the pair.

Lorenz' motives are, to himself, inaccessible. His rationalisation about these is to me unsympathetic. His attempt to proselytise I think a mistake.

People who do not keep animals will find in this book the pleasure they find in exotic lives, both the animals' and the author's. Some of his stories will rank with Barrington's part song for two Bullfinches and Darwin playing the bassoon to the climbing plant. Children may not like some of his standards : ‘ let the procuring of your pet cost some trouble or money, or both.’ Children believe that a reward is as sullied as a bribe. They value the luck that is deified in some cultures, and expressed by the *Magnificat* in our own. But they will be made happy by finding a person whose growing up was not ‘ fallings from us, vanishings ’.

But as this book is being widely read it is important to consider implications. It is badly translated, not only pompously, but with the misuse of words that is frequently a consequence. The two most serious are ‘ mimic ’ and ‘ mimetic ’ to mean ‘ exaggerated to function as communication ’, and ‘ highcaste ’ to describe an animal high in the peck order of its colony. Therefore it is difficult to discuss.

Lorenz uses several non-scientific verbalisations ; for instance the wind is stupid, trees wave their arms, and, most surprising, he tells us he is attempting to make a breed of dogs with ‘ an undistorted soul . . . which is specially capable of supplying that which poor, civilised, city pent man is so badly in need of ! ’ Every reader I have met has a different opinion of when he is using a poetic fallacy, and when he is being explicit. This is unfortunate, as one of his major contributions to our thought is the emphasis that a comprehension of human behaviour, like a comprehension of human anatomy, must be based on a knowledge of the vertebrate series. The functional significances of, for example, commuting and the Oedipus situation become clear by such comparison. He uses human words for animal activities—when they are respected in our community ; and natural history words for human activities if he wants to enlist sympathy for human frailty.

Lorenz has been criticised for changing his technical jargons as his ideas develop. In *King Solomon's Ring* he seems to tie himself in knots trying to avoid using one. Any discussion of the interaction of nature and nurture demands an explained use of terms, because everyday speech perpetuates archaic concepts. It is still literate to say I inherit my father's nose, his surname, and his watch !

Most of the theory could have been discussed so as to introduce the vocabulary used by Tinbergen.[1] Many people, certainly most adolescents, derive satisfaction from learning technicalities. Some of the terms of comparative ethology are becoming catch phrases. Children would like to use them correctly, and would look for the phenomena in their environment. Tinbergen has greatly stimulated people over eighteen. The book that will sell comparative ethology to people over twelve, who have every hope of making major contributions, has yet to be written.

The lack of bibliography is also disappointing. Three books are mentioned, two difficult, one impossible, to obtain.

This failure to provide any link with other authors seems most serious in his essay on the language of animals. His references to his tits and warblers suggests that he is working on the communicatory functions of song and call-notes. Perhaps he has obtained results that justify his ignoring previous discussions of the subject.[1] His inclusion of social insects in his generalisations about what is innate and ubiquitous, are not confirmed for hive bees. Von Frisch [2] does not know whether their symbolic dance which he calls a language is innate, and he has subsequently found geographical variation in it.

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[1] N. Tinbergen, *The Study of Instinct*, Oxford, 1951

The differences between Lorenz' generalisations and those of the other authors are that he underrates the extent of learning in an animal's lifetime, and geographical variation. This brings me to my most serious criticism of his thought processes, a consistent underemphasis on change. Much of his love of animals is for the individual differences between them, but his failure to draw certain conclusions from these has led many younger workers to consider that he does not believe in evolution. An example of their evidence occurs in the second half of the essay. This discusses the various ways in which animals and men communicate with each other, and concludes with several anecdotes of parrots and corvines *volunteering* human words in appropriate situations. These involve the recognition of animals, warnings, and departure. In all these situations birds use their voices among themselves. He describes a parrot that attempted to defaecate whenever looked at in its cage, because it was only liberated after this process.

However, Otto Koehler failed to *teach* a parrot to say 'food' when it was hungry, and Lorenz concludes: 'The whole complicated apparatus of the bird's syrinx and brain that makes imitation and association of thought possible, appears to have no function in connection with the survival of the species. We ask ourselves vainly what it is there for!' I can think of no adult bird that uses its syrinx when it is hungry, nor of any situation where it would be useful to do so. However, many birds, including the only wild parrots I have seen (*Psittacula krameri*) do use their voices after they have discovered food, thus attracting the flock. It might, therefore, be possible to teach a parrot to say the appropriate human words when it stole food, or when the appropriate fruits appeared on the trees each year. A parrot might learn to say 'look out' when it was startled visually, and then volunteer it when startled aurally or tactilely.

This underrating of the consequences of variation, and inhibition in looking for adaptations, results in Lorenz failing to recognise the ubiquity and speed of systematic genetic change, that is to say of evolutionary processes.

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[1] E.g. Julian Huxley, *Evolution. The Modern Synthesis*, London, 1942, pp. 305-309; and A. N. Promptov and E. V. Lukina, 'Conditioned-Reflectory Differentiation of Calls in *Passeres* and its Biological Value,' *Comptes Rendus (Doklady) de l'Académie des Sciences de l'URSS*, 1945, **46**, 382-384, and earlier.

[2] Karl von Frisch, *Bees, their Vision, Chemical Senses, and Language*, Ithaca, 1950, and earlier.

For example, page 30. The relatively enormous fins of the male in his drawings of the fighting fish *Betta splendens*, and his description of its fight, page 26 : ' The self-display dance can last for hours but, should it develop into action, it is often only a matter of minutes before one of the combatants lies mortally wounded on the bottom,' betray that he is considering the European and American domestic form. The dorsal and caudal fins of a *wild* male *Betta splendens* are of the same size as those of the female, and the anal fin is only slightly larger. The same proportions are found in the Siamese domestic fish that are bet on in fish fights. In these, the display lasts for seconds or minutes at most. The fish then fight, biting each other's fins, often reducing them to stumps. Only in some fights are the flanks and opercula injured. Wild fish never fight for more than twenty minutes, but domestic stock may continue for 24 hours. This extension of time has been produced by selection in the last century. Asiatic fish never kill each other or die of injuries ; their swimming may be affected temporarily by reduction of fin area. The defeated fish swims away and the victor does not chase him.[1] This is another example of the vanquished being able to call off a fight, as described by Lorenz, pp. 181-199.

Therefore the pattern of fighting which Lorenz describes, the exaggeration of the display, the introduction of a new movement, the violent ' ramming ' resulting in an internal rupture causing sudden death, or alternatively the lack of co-ordination between the contestants so that one is either unable to receive, or avoid a blow, let alone arouse a mercy inhibition, does not represent ' the result of an evolutional development of innate instinctive activities, at least a hundred times older ' than the ceremonial dances of Java. It has been developed in Europe in the last fifty years during a domestication by people whose motive for keeping such animals was the romantic one described by Lorenz on page 58. Such people would quite *unconsciously*, by the considerations with which they put fish together, and removed them from one another, *select for* display and *against* biting. The great fins may have been selected consciously, but the enormous *reproductive advantages* given by aquarists to the earliest fish in any shoal to become sexually distinguishable—of which Lorenz provides an example in his description of breeding cichlid fish—selects for precocity and exaggeration of any sexual dimorphism. Descriptions of the behaviour of the related *Macropodus opercularis* in the aquarium books of the 1870's similarly differ from anything we can observe today in European stocks of this species. Thus the fighting of wild fighting fish is more like that of sticklebacks than Lorenz emphasises, and all the differences are functionally correlated with the differences between the structures and durations of their nests.

Whenever the early records are full enough, or freshly caught animals have been examined, changes in behaviour have been discovered in stocks that have been kept in captivity, whether we think of them as domesticated or not. This failure to expect rapid evolutionary change with every change in the animal's environment has led Lorenz to his most questionable assertions.

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[1] Hugh M. Smith, 'The Fresh-water fishes of Siam, or Thailand,' Bulletin 188, United States National Museum, 1945

In his consideration of domestic dogs he attributes certain psychological characteristics to certain breeds. In my experience these correlations are not as vivid as they are to him. These differences he attributes to whether the breed has wolf ancestry : '. . . comparative research in behaviour has revealed the fact that all European dogs, including the largest ones, are pure Aureus and contain, at the most, a minute amount of wolf's blood. The purest wolf-dogs that exist are certain breeds of Arctic America. . . .' Aureus is the specific name of the Asiatic jackal. The quoted sentences reveal why Lorenz underestimates genetic change. If he is using blood to mean cellular antigens it is improper to write of the 'amount.' Cellular antigens are inherited discretely, and are *counted.* If 'amount of wolf's blood' is used as a vernacular for 'proportion of ancestors that were wolves 40,000 years ago', this is information that cannot be obtained by any technique, physical or psychological. The number of wolf ancestors of a breed and the number of wolf allelomorphs in a breed today are correlated, but this correlation is a function of the selection pressure. Genes from a single ancestor may spread through a breed, or species, if they give animals that carry them sufficient advantage. Also it is inefficient to think of 'number of allelomorphs.' Gene loci are not equipotential. Only the loci that are concerned with the differences which we have learnt to recognise between wolves and jackals can as yet be considered. Also gene loci are not stable. It is probably the most correct approximation to think of every single wolf, jackal, and dog that ever existed as having a different genotype. The expression 'wolf's blood' disguises the fact that every single wolf gamete is different, and in any change of environment, from the physiology of a specific hybrid, to being reared from puppy-hood by a human being, the differences are more important than the similarities. In two closely related species like the wolf and the jackal, variations indistinguishable by any technique (including the genetic) are expected to occur independently, as Darwin pointed out. This is implied by classifying them in the same genus. If the two species were put in the same environment similar adaptations to it would be selected in each.

The differences Lorenz emphasises between his Alsatian and his Chow can be explained by the recent histories of the two breeds, without hypothesising more allelomorphs derived from wolves in the latter. Alsatian dogs are used by armies and police to hunt other men, and over large parts of Europe they are sheep dogs. They lead the blind and become film stars. The draft dogs of Belgium are like them. They have been selected to co-operate with men—a continually changing team of men. They have been selected recently to look more wolf-like. This may once have been to make them frightening, now it is valued for romantic reasons, which Lorenz shares. Chows, until the opening of China to the west, were as much domestic animals as the rats in our warehouses, though they were more often eaten. Their coat and skeletal configuration, the latter indistinguishable from that of the dogs of the neolithic lake dwellers, were ecological adaptations. They lived in little groups, like other feral dogs.

His emphasis on hereditary continuity as opposed to genetic change is the reason why Lorenz omits certain details from his papers. This book is more scientific. Here he frequently tells us *where* he obtained his animals, and sometimes even *when*. These, the locality data of classical taxonomy, are among the pieces of information in a paper the value of which is most likely to increase with every reinterpretation. Lorenz either emphasises a unique occurrence, or makes a generalisation about the species implying that for the purposes of his argument all specimens behave alike.

Speciation geneticists are becoming increasingly interested in animals' perception of one another. No one could tell us more about this than Lorenz, but we can find no appreciation of our questions in his work. During the last twenty years increasing speculation has centred round the genesis of psychological sterility barriers. Lorenz seems to consider the question so little that he even shows aesthetic callousness to an animal, page 184. He put two doves together. The female pecked the whole of the skin off the male's back. Lorenz was so shocked by this that he builds a considerable theory on it, and draws the female with a sword under her wing (p. 198). There are many questions I should like to ask, but of the facts presented the relevant one seems that the doves were of species so different that Lorenz follows the taxonomists who put them in different genera ! To attempt to make a specific hybrid is always cruel to an animal, because failures in communication are to be expected.

I have tried to review this book, and not to discuss some of Lorenz' recent ideas.[1] The attempt by a scientist to present his work to a wider audience is very much the concern of any student of the philosophy of science, both for the light it throws on the scientist, and the consequences on the intellectual climate in which we live. The audience for which such a book is written contains one component incomparably more important than the rest, both for the author and for us, his successors. A few of these may start keeping animals because of this book, but most of Lorenz' successors will have kept animals since they were able to coordinate their eyes and their hands. For them the wish to keep an animal does not arise 'from a general longing for a bond with Nature.' They have no sense of separation ; they eat the dog's food to find out what it tastes like. The dog figures in their *monologue intérieur* as their brother or their husband. In large areas of present culture—which includes but is not confined to the English speaking world, but also does not correspond with the alliances of the 1939-1945 war—many of these people have become geneticists. They are surprised at Lorenz' repeated antitheses between man and Nature. I am uncertain whether it is fair to Lorenz to discuss this point. I suspect that it, like the sentimentality about dogs, is an insincerity put in because he thinks it will make his biological ideas, *which are in contradiction to it*, more acceptable to a hypothetical British Public. Unfortunately it blurs those of his ideas which I think important, while exposing those I think retrograde. In Austria discussion of evolution has been impossible, not only under the Nazis but under the Roman Catholic Church.

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[1] E.g. K. Z. Lorenz, 'The Comparative Method in Studying Innate Behaviour Patterns,' *Symposia of the Society for Experimental Biology IV*, Cambridge, 1950. The section ' " Comparative anatomy " of behaviour ' shows how he is handicapped by lack of hypotheses, Lamarckian or selectionist, about the causes of evolution.

His demonstrations that much of our snobbery, sense of property, and sexual morals are shared by other animals, in the form which we regard as specifically human, could have been emphasised, as this is underestimated by current moralities (e.g. the Christian, the Marxist, and the Freudian). I see no reason why in *Homo sapiens* the instinctive signs which transmit mood should have 'degenerated as our word-language developed.' They are still relied on in the activities of preservation of the species, love-making, the rearing of children, clinical medicine, human physiology, team work especially in the presence of danger, and oratory.

The study of speciation by natural selection, which may be described as the contemplation of death, produces more sense of community with other forms of life than does the study of behaviour, which, as it relies on the observation of inappropriate actions, either apparently accidental or provoked, may be described as the contemplation of sin.

H. Spurway

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Johannes Kepler : Life and Letters, Carola Baumgardt (with an introduction by Albert Einstein), Philosophical Library, New York, 1951. Pp. 209. $3.75.

Although this book throws little light on Kepler's astronomical and mathematical researches it is to be welcomed for two reasons : any addition to the scanty literature in English on this strange and fascinating genius would be worth having ; moreover, the present work has a more positive claim to our gratitude, for it provides, largely through the medium of his own letters, a detailed picture of the man against the background of his time.

As Einstein remarks in the introduction to the book, we meet in these letters ' a finely sensitive person, passionately dedicated to the search for a deeper insight into the essence of natural events.' A prolific originator of hypotheses concerning the physical universe, Kepler was remarkable for the way in which he subordinated his mathematical inventiveness to the stern discipline of observational confirmation. In a letter written by him after his first visit to Tycho Brahe, he says, ' One of the most important reasons for my visit to Tycho was the desire, as you know, to learn from him more correct figures for the eccentricities in order to examine my *Mysterium* and the just mentioned *Harmony* for comparison. For these speculations *a priori* must not conflict with experimental evidence ; moreover, they must be in accordance with it.'

Kepler was perhaps the supreme instance of a natural philosopher with one foot planted in the medieval world and one in the modern. For example, in his *Harmonice Mundi*, published in 1619, we find the third of his immortal laws buried like a jewel in a mountain of ' metaphysical, poetical, religious and even historical speculations,' including the attribution of the different ranges of voice, bass, tenor, etc., to the various planets!

Carola Baumgardt has made an interesting selection from the vast mass of letters written by Kepler and has strung her selections together on a biographical thread. The book ends with extracts from letters by several contemporaries giving an account of Kepler's last days. On the whole, the author's translation reads easily, although in one passage (p. 124) something has obviously gone wrong for reference is made to ' the wavelengths of certain planets.'

It is to be hoped that this selection of material relating to Kepler will inspire someone to give us a more ambitious book in English on his life and work.

G. J. Whitrow

Short History of the Art of Distillation, R. J. Forbes, E. J. Brill, Leiden, 1948. Pp. 405. 18 guilders.

Distillation is the oldest and one of the most important methods for the preparation of pure chemical substances. This art, for its principles were not fully understood until the nineteenth century, contributed greatly to the developing sciences of chemistry, metallurgy and pharmacology, and in the preparation of alcoholic beverages and perfumes it constituted one of the earliest chemical industries. Professor Forbes traces its history from antiquity to the end of its empirical development, when the continuous distillation column was perfected in France between 1820 and 1840, only then to be surpassed through the application of mathematical theory in the last decade of the century.

Forbes first discusses the disputed evidence whether primitive peoples in antiquity knew the art, and passes on to a detailed account of the chemical techniques, including distillation, known to the chemists of the Alexandrian epoch, stressing that through the lack of the mineral acids, reactions were carried out in the molten state, never in solution, and that because of insufficient cooling only liquids with boiling points higher than that of water could be recovered efficiently. The Arabs developed extensively a steam distillation technique for perfumes and essential oils which led to thriving trade at Sâbûr and later Damascus. Yet dry distillation methods were still dominant in their pharmacies, and drugs from animal materials were considered more potent than those of vegetable and mineral origin. In the Middle Ages the mineral acids were discovered and were soon applied in metallurgy, alcohol was prepared for the first time by distillation, and improved cooling techniques led to distillates becoming much more prominent than the powders, syrups and decoctions of scholastic medicine. The distillation books of Brunschwygk in 1500 and 1512 inaugurated a whole series of treatises throughout the sixteenth and seventeenth centuries. Slowly the techniques were improved. Boyle designed an apparatus for laboratory vacuum distillation, but it was not exploited until much later. Even at the end of the eighteenth century, Chaptal remarked that the methods of fractionation and condensation on a large scale were still bad and there was no proper means of regulating the furnace, but this was soon remedied by the French distillers.

The book follows this survey in a continuous story written around the men who developed the art. Short biographical notes illuminate the social setting, and the descriptions, analysis and illustration of their works provides one of the most attractive aspects of the book, for so many of their works have now become exceedingly rare. There is a wealth of fascinating detail here. There are 203 illustrations and the bibliography covers 673 books, articles and patents : 70 per cent. of this is original source material and the rest largely works of scholarship.

Two important themes appear in the story. First, there is the role of distillation in technology and the stimulus to further refinements of technique that the requirements of technology demanded. Forbes has gathered together a great deal of information on the distilling trade since the Middle Ages, ranging from the changing public taste in drink and the rise and fall of various distilling centres throughout Europe, to the search for new raw materials and the construction and perfection of large scale plants. Besides the distillation of alcohol, he also discusses the production of wood and coal tar, the distillation of petroleum, and the preparation of the mineral acids. Secondly, there is the part played by ideas about distillation in the general development of chemical thought and the physical sciences. Whilst the result of distillation (the separation of materials), had been well appreciated for a long time, other processes achieving the same end, filtration and sublimation, were considered under the same heading. The result of such a process and its actual mechanism could not be differentiated until there was a clear understanding of the states of matter. Another problem (put in modern terms) was whether distillation effected a physical or chemical change, and intimately connected with this were the conflicting views on the nature of fire, whether it was a material thing or a physical agent. This was only to be fully elucidated towards the end of the eighteenth century with the introduction of concepts of specific and latent heats, the work on latent heat owing much to evaporation and heating problems in technology.

Although distillation was the favourite operation of the alchemists, there is only passing reference to their activities. The lack of any precise knowledge of their recipes except for a few instances makes it a speculative study, out of place in relation to the detailed factual account which Forbes is concerned with. Mention might, however, have been made of Boerhaave's many distillations of mercury, 877 in all, undertaken in his investigation which proved that ' fire cannot increase metals or quicksilver by concreting with them, or generate anything metallic, or form any durable change of the metal '. The corresponding ' transformation ' of water into earth by repeated distillation, later shown to be false by Lavoisier, is described in some detail.

In his introduction, Professor Forbes remarks that no proper history of distillation from its origin up to the present time had been written. Only one of the three previous histories, which cover parts of the subject, is in English. His is an excellent book and this serves to emphasise its great value.

Philip George

Scientists and Amateurs : A History of the Royal Society, Dorothy Stimson, Sigma Books, Ltd., London, 1949. Pp. xiii + 270. 15s.

Miss Stimson, chairman of the History Department of Goucher College, Baltimore, has given us a very interesting and readable account of the origins and rise to power of the Royal Society. Naturally there is little which concerns either the method or the philosophy of science, but as an example of the worldly difficulties attending the advancement of 'natural philosophy' it is most enlightening. The two chief troubles appear to have been finance and enthusiastic amateurs. Without the latter the Society could never have started, and the financial difficulties would have been insuperable, and yet it was publications of the amateurs which were largely responsible for stimulating the ridicule, both in verse and prose, on the stage and elsewhere, with which the virtuosi were assailed. Those who have tried to introduce scientific method into psychical research in recent times will read with sympathetic insight of the teething troubles of the world's oldest scientific society in continuous existence ; and will note with dismay, that in three centuries human nature has changed but little.

G. BURNISTON BROWN

A Treatise on Induction and Probability, Georg Henrik von Wright, Routledge and Kegan Paul, Ltd., London, 1951. Pp. 310. 30s.

PROFESSOR VON WRIGHT in this book first discusses the nature of the problem of inductive inference and the requirements of a theory in order that it may answer the problem of Hume. He distinguishes between three main problems : (1) the mainly psychological problem of discovery and of the origin of inductive inferences in science, (2) the logical problem of analysing the inferential mechanism of induction and (3) the specifically philosophical problem of justification. He is concerned mainly with the second of these problems.

The chapters are entitled : Induction and its problems ; preliminary considerations on logic ; the form of inductive inferences—laws of nature ; induction and elimination ; induction and deduction ; induction and definition ; the logic of probability ; probability and prediction ; probability and laws of nature ; induction and inverse probability.

The first chapter deals with formal logic on similar lines to Wittgenstein's and throughout the book theorems are proved with extreme care. The methods of induction by simple enumeration and by elimination are well discussed. A long section deals with alternative interpretations of ' A causes B ' as ' A is a necessary condition of B ', ' A is a sufficient condition of B ', and ' A is a necessary and sufficient condition of B ', and of the kind of data that would be needed to establish any of these forms in a given instance. This leads to a discussion of the nature of scientific definitions and of the parts played in them by convention and inference.

Six axioms of probability theory are stated and the principal general theorems are derived in extreme detail. On pages 181-2 the author starts with a form of the multiplication rule and takes a whole page to derive two consequences that follow at once by division by non-zero quantities.

The interpretation and use of the formal system are discussed at great length, but the results are few, and the author alludes to many important topics only to say that they will not be discussed. He classifies possible interpretations in terms of possibilities, frequencies, and degrees of belief. He shows that within a certain domain a method of elimination on deterministic lines (including classical determinism but not confined to it) can be stated as a form of possibility theory. I very much doubt whether there are any scientific laws that could be established in this way. Continuous distributions are totally unconsidered. But most physical laws depend on the normal law of error or on some other law with an infinite range, so that no finite set of observations would demonstratively eliminate any possibility at all. Even for finite distributions I doubt whether any law of the form 'all A is B' is ever established.[1]

The author thinks that a limiting frequency theory can be made to work, but does not appear to recognise to what an extent special assumptions about the existence of limits have to be piled up to make the axioms true on that interpretation.[2]

The book never gets to grips with the problem of rational belief. There is much talk about the 'psychological' interpretations, with the usual arguments about disagreements between people's beliefs and the difficulties of measuring degrees of actual belief. It is really preposterous that at the present stage of development of the subject this should be thought worth discussing. Even within deductive logic experts have held false beliefs for ages, for instance that every continuous function is differentiable. On a much lower level, but nearer to the level of ordinary guesses about probabilities, I once got 29 different answers to a numerical question in an examination. It has never been urged, so far as I know, that mathematics should include the theory and classification of wrong answers. Von Wright does appear to think that something might be said for a theory of rational belief (though he appears to have failed to see a somewhat elephantine joke of De Morgan on the difficulty of distinguishing between what one does think and what one ought to think); he thinks (p. 234) that rational belief is defined in terms of probability and not conversely, but omits to notice that *some* explanation is needed to give the reader any idea of what he is talking about. It is not enough to say only that something exists that satisfies the axioms. On page 244, we have 'Estimates of Inductive Probability are notorious for their vagueness and subjectivity'—thus reverting, apparently, to an actual belief theory. It would have been reasonable to expect that he might at least mention the fact that considerable efforts have been made to remove this defect, apparently with some success. The author decides that the importance of the inverse principles is largely an illusion. This is not very surprising in the context, because he has omitted all problems where they do lead to important results. Even the theory of estimation has been restricted to cases where only a finite set of chances are possible; and significance is not mentioned at all, so that the question of the establishment of quantitative laws is never approached.

HAROLD JEFFREYS

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[1] Cf. *Mind*, 1950, **59**, 313–319

[2] Cf. *Nature*, 1950, **166**, 413 (Review of H. Reichenbach's *Inquiry into the Logical and Mathematical Foundations of the Calculus of Probability*)

Science and Humanism. Physics in our Time, E. Schrödinger, Cambridge University Press, 1951. Pp. ix + 68. 8s. 6d.

This small but profound book contains the substance of four public lectures delivered under the auspices of the Dublin Institute for Advanced Studies under the title 'Science as a Constituent of Humanism'. The author begins by asking what is the value of scientific research, either in a narrow specialised field or in a wider sense as the sum total of achievements in all fields of science, and answers by saying that its value lies not in technological advance, which is a doubtful blessing, but in its contribution to the solution of the problem 'Who are we ?'. The aim of these lectures is to show what is this contribution in the field of modern physics.

He begins by describing something of the radical change that has taken place in our ideas about matter and how it is that a particle is now regarded not as a permanent entity that retains its 'sameness' for ever, but as an instantaneous event. Form or organisation (*Gestalt*), not substance, is the fundamental concept. He refuses to discuss whether the picture we have made for ourselves of, say, glowing iron vapour in terms of particles moving in space is true or false ; all we can discuss is its adequacy ; and the picture never can be complete, because of the impossibility of continuous observation. All this is illustrated by clear descriptions of experiment, by analogy that is not overdrawn, and finally by a brilliant example based on triadic fractions, so lucidly expounded as to make it seem simple.

He shows how wave mechanics can be invoked to provide a continuous description of something, but it still leaves a gap ; the gap between the wave picture and the observable facts.

In the next part of the book he asks whether the difficulties involved in the new physical ideas are really inherent in objective nature or whether they mark a stage reached by the human mind in its understanding of nature, and considers arguments for and against the postulate that a physical object can not only never be observed without interference, but can never even exist independently of the observing object. He strongly denies the suggestion, never made, he thinks, by a classical scholar, that the ancient thinkers made a mere 'lucky guess' in formulating the atomism of matter. But Democritus was certainly also aware that the whole picture of the atoms and the void was based on sense perceptions and that ultimate truth remained deeply in the dark.

How far does the new physics provide an answer to that perennial problem of the apparent contradiction between mechanical determinism and our conviction that we possess free will ? Classical physics rested entirely on the principle of causality, but now there are new, unprecedented difficulties as regards causation. Can free will bridge the gap of indeterminacy ?

A careful review of the considerations put forward for and against this hope ends with the verdict that quantum mechanics has nothing to do with the problem of free will, if such a problem there be.

KATHLEEN LONSDALE

The History of Science. Origins and Results of the Scientific Revolution, A Symposium, Cohen & West Ltd., London, 1951. Pp. 184. 8s. 6d.

THIS is a collection of talks to Sixth Forms, broadcast by the B.B.C. during 1949–50, and later repeated on the Third Programme. Professor Butterfield opens with 'Dante's view of the universe', and is followed by Professor Postan, asking: 'Why was science backward in the Middle Ages?'—a question which begs the historical question which should have preceded it, and concludes with the curious statement: 'What [the men of the Middle Ages] achieved in advancing the practical arts of humanity or in preserving and transmitting ancient learning, they did in so far and as long as they were not typically medieval.' Next come Professor Dingle on Copernicus, Professor Broad on Francis Bacon, and Sir Henry Dale on Harvey, the last very engaging. Dr S. Lilley follows with a sociological thesis about the development of scientific instruments in the seventeenth century, supposing that before that time scholars never used their hands, a thesis contradicted by many examples from the preceding five-hundred years. Professor Butterfield then takes up the subject of Newton and his universe, followed by Professor Willey on the effect of science on the general educated outlook in the seventeenth and eighteenth centuries, Dr D. McKie on Lavoisier and his predecessors, Dr Sherwood Taylor on early nineteenth century physics and chemistry, Dr Hugh Clegg on Pasteur, and Dr C. F. A. Pantin and Professor C. E. Raven with two talks on Darwinism and its influence. The tail of the book is brought up by three talks by Dr J. A. Ratcliffe on the study of electricity in the nineteenth century, by Sir Lawrence Bragg on the atomic theory, and by Professor M. L. Oliphant on 'Science today'. Altogether the series makes pleasant reading; it is not in any way original, but is well suited to its original purpose.

A. C. CROMBIE

The Origin, Variation, Immunity and Breeding of Cultivated Plants, N. I. Vavilov. Selected writings translated from the Russian by K. Starr Chester, *Chronica Botanica*, 1951, **13**, No. 1/6. Chronica Botanica Co., Waltham, Mass. and Wm. Dawson and Sons, Ltd., London. Pp. viii + 366. $7.50.

IT is curious that Vavilov's special interest for the philosophy of science resides in the fact that he was not very interested in philosophy. Before 1935, Vavilov was chiefly known as one of the leading authorities on crop plants, as the organiser of the long series of Soviet expeditions to collect initial material for plant breeding, and for his theories on gene centres and homologous variation. It is probably true that his theories did not mark any very fundamental stage in the development of genetical theories. In some respects they had been anticipated by earlier writers ; in many respects later work has necessitated their modification ; but Vavilov did bring forward an immense body of factual data, which, as regards details at least, has placed all subsequent students of crop plants in his debt.

When Lysenko burst in upon the Soviet genetic stage, Vavilov was President of the Lenin Academy of Agricultural Sciences and Director of the Institute of Plant Industry, and he acted on many occasions as the spokesman of classical genetic theory. He gave a rather lukewarm welcome to Lysenko's theory of phasic development, but criticised the scientific basis of Lysenko's new genetical theories. Vavilov's monumental work on the *Theoretical Basis of Plant Breeding* was appearing just as Lysenko was getting under way, and brief allusions to the new genetics appear on p. 277 of Dr Starr Chester's translation.

Vavilov's later history is now well known. He opposed Lysenko at a series of Soviet genetical congresses, always arguing on a factual basis, but made no apparent impression on the Soviet public, who were unanimous in their preference for Lysenko. Finally Vavilov was dismissed his posts and died under obscure circumstances about 1942.

Dr Starr Chester has brought together, in the present volume, translations of some typical articles by Vavilov, namely 'The Phytogeographical Basis of Plant Breeding,' 'The Law of Homologous Series in the Inheritance of Variability,' 'The Study of Immunity of Plants from Infectious Diseases,' and 'The Scientific Bases of Wheat Breeding.' For those not conversant with Russian, this selection provides a good introduction to Vavilov's writings. The translation is, on the whole, exact, and perhaps even over-faithful to the original. Vavilov's style was not brilliant ; it was in fact laboured and involved, and this quality is apparent in the translation. The rendering could possibly have been improved with regard to varietal names, which appear to have been translated, backtranslated or transliterated according to no very obvious scheme.

What is of particular interest for philosophers of science is the clear characterisation of a perfunctory Marxist scientist. Vavilov does indeed include some conventional allusions to the effects of capitalism on science (p. 3) and to the dialectical unity of theory and practice (p. 4), and some vaguely eulogistic references to Michurin (p. 15) and Lysenko (p. 53). These, however, have no perceptible effect on the rest of the book, and it is clear that Vavilov's enthusiasm is primarily for scientific data, and not for philosophical or sociological excursions. It is only too probable that, even in his own field, Vavilov was more interested in the theoretical development of his facts than in applying them to the breeding of improved crops, and that his lack of interest in the sociological role of science led to his downfall.

The pure scientist has worked under highly favourable conditions in Western Europe in recent times. Dr Starr Chester's delineation of Vavilov's scientific attitude illustrates one way in which an enthusiasm for pure science may lead to difficulties.

R. H. Richens

Eddington's Principle in the Philosophy of Science, Sir Edmund Whittaker, F.R.S. (The fifth Arthur Stanley Eddington memorial lecture), Cambridge University Press, 1951. Pp. vi + 35. 2s. 6d.

The choice of Sir Edmund Whittaker as Eddington lecturer for 1951 was singularly felicitous. Whittaker supervised the publication of Eddington's posthumous book, *Fundamental Theory;* he understood its argument better than most of us, and shared Eddington's mystical and reverent approach to nature.

Eddington's principle, taken by Whittaker as the subject of his lecture, was stated by him as follows : ' All the quantitative propositions of physics, that is, the exact values of the pure numbers that are constants of science, may be deduced by logical reasoning from qualitative assertions, without making any use of quantitative data derived from observation.' So stated, the principle is reasonable, being the fundamental faith of most theoretical physicists ; but it may be doubted whether Eddington would have been satisfied with this statement. He often averred that when we discover laws of nature we in fact only discover the laws of our own thinking. That is, he asserted that the fundamental qualitative laws themselves have compelling logical force. Others might disagree, and suggest that the compulsion of Eddington's beliefs was mystical and not directly logical. A frequent criticism of Eddington's books was that a ' logical ' deduction of a physical law was often found to depend on an apparently innocuous remark artfully introduced a hundred pages earlier. With this in mind, it may appear best to state Eddington's principle as Whittaker has done ; but something of Eddington's faith seems to be lost.

Whittaker recaptures something more of Eddington's spirit in his discussion of the vitality and unifying power of the concepts of theoretical physics. At the same time, he at one point finds a difficulty. Eddington's principle appears to suggest that the conclusions of theoretical physics can be permanent, even though based on qualitative conceptions that are bound, later, to be found to be incomplete. Whittaker does not completely get over this difficulty ; but he is glad to point out that a large group of the qualitative assertions of theoretical physics are not likely to be superseded.

These are the 'principles of impotence,' like the second law of thermodynamics or Heisenberg's indeterminacy principle, which assert that nature does not permit us to do certain things. The importance of such principles has been indicated earlier by Whittaker; they were not, however, specifically mentioned by Eddington.

The lecture concludes with comments on Eddington's cosmology and its rivals. Whittaker is glad to note evidence favouring Eddington's version, because it fits in with his own beliefs about the origin and purpose of the universe. He conceives a deity like Jeans' mathematician God; changes in the universe are subject to permanent mathematical law, and since mathematical law is a concept of the mind, our minds have access to something akin to themselves behind the universe. I personally sympathise with, and in large measure share, Whittaker's views; but I feel that here he has omitted something from his arguments. A belief in a mathematical God possessing the nature of an electronic computer is hardly satisfying! One feels that here Whittaker has supplied something from the mysticism which he shares with Eddington; he finds a sacramental quality in nature, because his own approach to it is reverent.

T. G. Cowling

Francis Bacon: Philosopher of Industrial Science, Benjamin Farrington, Lawrence & Wishart, Ltd., London, 1951. Pp. viii + 198. 12s. 6d.

The author in his preface to the English Edition complains that it has become fashionable to convert Bacon from a reformer of society into a reformer of logic, so he has attempted to define and defend Bacon's own conception of what he tried to do and wanted done. This, he maintains, 'has been grievously obscured—desecrated would be his (Bacon's) own word—in the philosophic tradition of his own country. Too often have Bacon's commentators ignored, or still worse, apologised for what Bacon called his only earthly wish, namely, to improve man's lot by extending his dominion over nature. If to be rescued from this attitude be a thing to be grateful for then I owe it principally to Marx, who not only claimed for Francis Bacon his rightful place as the founder of English materialism but also disclosed the bearing of his thought on the industrial revolution of his age.' At the end of the book we are told that this attitude 'has ruined Baconian scholarship in England.'

The present reviewer has carefully re-read his own chapter on *Francis Bacon* in a recent book, which was founded on the fruits of Baconian scholarship in England as well as on the edited works, without, of course, the assistance of Professor Farrington, or of Marx, and he cannot bring himself to believe that such a false picture emerges, tainted with ruin and desecration. It is true that he preferred to head his chapter with a quotation from the *Novum Organum :*

> Certainly the contemplation of things as they are, without superstition or imposture, error or confusion, is in itself more worthy than all the fruit of discoveries,

rather than a quotation from a letter, which Professor Farrington uses on the title page :

> To write at leisure what is to be read at leisure does not interest me. My concern is with life and human affairs and all their troubles and difficulties. It is these I wish to improve by true and wholesome thoughts.

But that was due to the reviewer being an active scientist and so having, perhaps, a more sympathetic insight into Bacon's character than the learned Professor of Classics at Swansea (Professor Farrington) or that alien to Western culture, Marx.

The author candidly admits that he has not hesitated to make such alterations in citing Spedding and Ellis's *Collected Works* ' as suited my own taste in translation, and have occasionally preferred to make an entirely fresh translation.' The present reviewer has not ferreted out these alterations, and would not be competent to judge them in any case, but the translation of *Novum Organum* as *The New Logic*, seems to him as serious a distortion as the author regards the change in title of the whole design, from *The Great Instauration* to that of one of its parts, the *Novum Organum* (which arose in Holland apparently).

Organum, surely, is a Latinised form of *Organon*, and this meant an instrument for obtaining new knowledge, involving, even in Aristotle's Lyceum, observation, and in Bacon's Bensalem, observation and experiment—that is to say, for more than mere logic, which Bacon, of course, despised.

It would certainly be helpful if Professor Farrington would make an entirely fresh translation of the sixth of the *Proximate Instances, wanting the Nature of Heat* (*Nov. Org.*). It is confusing as it stands and has led him to the error on p. 125 where he says St. Elmo's Fire is phosphorescence in the sea.

Professor Farrington has put up a bogey and knocked him down again, but he does it with great charm and his book is eminently readable.

G. Burniston Brown

The Structure of Appearance, Nelson Goodman, Harvard University Press, Cambridge, Massachusetts (London : Geoffrey Cumberlege), 1951. Pp. xv + 315. 25s.

THIS book is the fruit of research begun by Professor Goodman in 1930 and is a revised, expanded and rewritten version of *A Study of Qualities*, which was his thesis for a doctorate at Harvard. In the words of the Introduction ' it attempts to use the techniques and example of modern logic in the investigation of problems of philosophy.' It is, as the title suggests, a study of phenomena and of phenomenalistic systems but the author is careful to point out that this does not imply that he advocates phenomenalism—and neither does his nominalistic approach imply that the acceptance of the system outlined depends upon an acceptance of nominalism. The process of ' platonising ' the system is easier and more obvious than the reverse process.

The book is divided into three main sections. In the first there is a discussion of the general principles of the theory of systems in the course of which extensional isomorphism between definiens and definiendum is accepted as the criterion of a definition, the calculus of individuals to be used later is outlined and some important conclusions about simplicity are reached.

The second section outlines two systems : that of Carnap's *Der Logische Aufbau der Welt* and a system built along somewhat different lines, which is to be adopted and developed throughout the rest of the book. Professor Goodman here makes three distinctions. A system may be (*a*) *phenomenalistic*, taking as basic units qualia, presentations, etc., or *physicalistic*, taking things, processes, etc. ; (*b*) *particularistic*, taking as basic units concrete spatially or temporally bounded particulars such as phenomenal events, or *realistic*, taking non-concrete qualitative elements such as qualia. This second division is a division within phenomenalistic systems and each of these may be (*c*) either *platonistic*, admitting non-individuals as entities, or *nominalistic*, admitting only individuals.

Carnap's system, then, is given as an example of a particularistic system which is also platonistic, for it uses the calculus of classes. Only that part of this system which deals with phenomena is here discussed, but both the exposition of the system and Professor Goodman's comments on it are exceedingly valuable. Professor Goodman's own accepted system, on the other hand, is an example of a realistic and nominalistic system and this too is given with great clarity and conciseness. It is obviously impossible to comment on these in detail here.

The third section deals, in the terms so far defined, with problems of the ordering of qualities and the derivation, through such ordering, of shape and size predicates. There is also an interesting account of time, duration and eternity.

Unfortunately, however, praise cannot be unalloyed. We are entitled to expect, from the Introduction and the publisher's note on the dust-jacket, that we are to be shown how certain philosophical problems of a non-mathematical kind have been, or might be, solved by the application of symbolic calculi and this many philosophers would welcome. But the expectation is not fulfilled. We are in fact given a number of skilful and highly illuminating discussions, in ordinary language, of certain problems, which lead to further discussions of the ways in which the features of these problems may be incorporated in the system. But the clarification which is done is mainly presystematic and is essential to the construction of the system. The problems which interest the author largely concern the ways in which certain characteristics of our experience and the problems they raise may be precisely represented in logical calculi rather than how these latter problems may be solved with the help of the calculi. The book thus seems to fall unintentionally into two parts. The discussions of philosophical problems in ordinary language do not appear to require the expression of these problems in the system and might well have been done without it. The language of the system does sometimes serve as a useful shorthand but this is surely insufficient justification for its use and such justification must lie beyond the present work : we have seen what has been put into the system, how it is constructed, and it remains to be seen what can be got out of it, how it may be used. Probably, indeed, it is Professor Goodman's intention to go on to show that it may be used to solve philosophical problems once the preliminary business of accurately stating them in the system has been accomplished.

However, as I hope I have made clear, to say that the book does not do all it claims is to cavil, considering how much it does. Both the extra-systematic discussions and the fitting of them into the system are excellent in their own way, the first, in particular, throwing much light on the problems involved. That the field covered is narrow was inevitable in view of the detail in which they are discussed. If the problems appear to some to be minor ones, and therefore unimportant, this is a pity, for they are problems which it is essential to clarify before the major ones can be attacked.

I noticed a number of misprints, most of which will not mislead, but the following are probably worth mentioning : p. 166 (footnote) : ' II, 3 ' should be ' II, 4 ' ; p. 180 : the plan of numbering definitions outlined on p. 43 here seems to have been abandoned. ' D7.03 ' should be, according to this plan, ' D7.031'. On p. 182 ' D7.03 ' and ' D7.031 ' should be ' D7.032 ' and ' D7.033 ' respectively ; p. 181 : the reference in line 17 to D7.02 should be to D7.03 ; p. 183 : the same departure as that on p. 180

from the original plan of numbering occurs here. 'D7.04' should be 'D7.041', and so on; p. 267, line 21: for '61' read '41'; p. 280, line 12: for 'c' read 'b'.

Peter Alexander

The Development of Logical Empiricism, Joergen Joergensen, International Encyclopedia of Unified Science (Vol. I and II: Foundations of the Unity of Science), Vol. II, No. 9, The University of Chicago Press, Chicago, Illinois, 1951. Pp. iii + 100. $1.25.

This monograph gives a short history of the philosophic views of the Vienna (and related) schools from its beginnings in 1923 until the outbreak of war. A brief postscript by Norman M. Martin consists mainly of a selected bibliography of American publications up to 1949.

Logical empiricism has been a fruitful, and perhaps the most important movement, in modern philosophy. Our knowledge starts with experience and is formulated in sentences which must be analysed by means of mathematical logic; and this method applies not only to scientific but also to everyday language. The ideas of Mach, Russell, Wittgenstein, and Carnap are given in some detail, mainly by the help of quotations. The work of the Berlin group, of the Polish school of logicians, and of the pragmatists is more briefly described. It was the merit of this movement to have recognised that a *criterion of meaning* is needed if language is to be a reliable instrument for communication. Earlier versions of this criterion, in terms of verifiability and testability, are presented. Finally, the thesis of physicalism and the prospects for a unified science are discussed.

The account given in this pamphlet is as accurate and complete as can be expected in so few pages. There are, inevitably, some mis-statements and omissions. Logical empiricists have always rejected dialectical materialism as a metaphysical doctrine belonging to the mid-Victorian era. The discussions about probability, causality and induction which took place before the war are not given a sufficient treatment. And, in general, the the work done in logic which was, in many ways, the main achievement of this movement is rather neglected. It is a pity that the monograph is not up-to-date; but this is presumably due to the conditions under which it was written.

It is of great interest, however, to see how this movement arose from traditional philosophy. The scientists and philosophers who co-operated in this work did not want to found a new philosophic system; but being influenced still by epistemological doctrines they made many, though often useful, mistakes. Gradually, the movement seems to succeed in liberating itself from all -isms to approach, through semantics, a methodology of science. This further development is hardly mentioned in the monograph.

E. H. Hutten

Mind, Life and Body, Reginald O. Kapp, Constable and Co., Ltd., London, 1951. Pp. xvii + 196. 12s. 6d.

PROFESSOR KAPP is a scientist and an engineer and quite properly scornful of attempts to solve problems in science by methods that are not scientific. In this book he asserts, and claims to show, that the mind-body problem is a real and important problem to be solved by scientists rather than philosophers. That is, it is open to investigation by the scientific method of observation and experiment, and cannot profitably be dealt with by the speculative method as is shown by the failure of all the philosophical assaults on the problem to produce an agreed solution. It is a matter of causality ; scientists ' alone are concerned with the nature of causality ' ; the problem is to be discussed here only in terms of causality. It follows, of course, that Professor Kapp is scornful of most of the rest of us, whether scientists or philosophers, for our attitude to the problem.

There are many points in the course of his argument which could be questioned, but it is perhaps more profitable in a short review to consider only his fundamental contention that the mind-body problem, as he states it, is a problem for scientific investigation and the basis of a ' vast new field for research ' which he proposes to call ' Diathetics ', the study of ' diathetes ' or ' non-material influences '.

Matter is defined as ' everything with location '. The problem then becomes ' Do things that lack location ever act on things that have location? ' There is a number of connected questions which must also be answered, but this is central and is sufficient for our present purpose. If you answer ' No ' to this question you are, in Professor Kapp's language, a ' monist ' ; if ' Yes ', then you are a ' dualist '. There are difficulties involved in both views ; he outlines the chief of them as he sees them and concludes, largely because of the difficulty of accounting for planning for the future and purpose without reference to minds, that the universe is dualistic, that life and mind are active realities without location and that they act on matter. Thus, it is in principle impossible to regard all the sciences as branches of physics, since some of them must ' take causes into consideration that are not physical forces.'

It must be said, in passing, that his argument about purposes and planning does justice neither to his own nor to the ' monist ' view because he fails to state in detail, and so to criticise in detail, the monist account of purpose and planning. We cannot, he says, describe purposive action without the use of future tenses ; ' . . . for one set of causes of a teleological event one must look for that in which the purpose and the anticipation reside ' ; and ' . . . a teleological event always has two sets of causes. It is doubly determinate. . . .' But here he is less than fair to the monist view that anticipation may be simply a mechanical consequence of past experience, so that phrases such as 'anticipation that certain conditions *will* obtain' use the future tense in a trivial sense to which great importance cannot be attached in this context. There are good reasons for thinking this view unsatisfactory, but Professor Kapp has not given them.

This is off our main point. It suffices for us that he reaches dualistic conclusions and then proceeds to hold that 'non-material influences' come within the field of investigation of science. By arguing from the nature of relays, mechanisms for controlling the moment at which energy shall be transmitted by other mechanisms without themselves transmitting energy, he concludes that the function of mind or life is to control the moments at which nerves, muscles and hands perform their various operations. Hence there must be, somewhere in all organisms, tiny mechanisms of which the operating or controlling elements are non-material, i.e. lack location, while the controlled elements are material, i.e. have location. The existence of these sophisticated pineal glands is reached by an argument similar to that for a first cause. The path between, say, the hand and the brain is regarded as, *inter alia*, a series of relays whose function is control, and whose operating and controlled elements are mechanical and so observable. But what about the first relay in the series? Unless we are to have an infinite regress, this relay must be controlled 'by means that differ in principle from the means by which every other relay in the set is controlled.' Professor Kapp tentatively puts forward the hypothesis that it is controlled by 'non-material influences' or 'diathetes', but this is tentative because scientific methods can verify or falsify it. The fundamental relevant question for the scientist, then, is, 'How and by what is a primary relay controlled?'

To discover how primary relays function scientists must look in 'the places where interaction occurs.' Apart altogether from the difficulty of knowing what is meant by an influence without location acting 'at a place', the scientific method to be used in discovering how these primary relays function remains mysterious. The basis for the scientific method, even for Professor Kapp, is observation and experiment and the only things that can be observed 'by physical means' are things that 'send out signals'. The only things that can send out signals are those with location so that, by definition, the operating element of the primary relay is beyond the reach of physical observation although the controlled element is not. Yet this is 'a question of mechanics'. How we can discover how the controlled element works, when it is only the latter end of a relay whose other end is beyond our reach, I fail to see. Of course, the phrase 'observation by physical means' suggests that there may be other means of observing, but no hint is given about what these are. The phrase 'indirect observation' is also used without explanation. This may be a clue, but to say that a thing is indirectly observed is usually to say that it is *not* observed but postulated to account for what is observed.

A further problem for the would-be diatheticist is that of knowing when he has reached a primary relay. Inability to discover the operating element is poor evidence for its being non-material. But Professor Kapp gives criteria for recognising the controlled element of a primary relay. The specific feature, and the most suspicious, is, I think, this: '. . . the moment of time when the minimum kinetic energy is acquired can be controlled by an influence without location.' This is based on the Principle of Uncertainty and

> suggests that there is a substantial uncertainty in the moment of time when the operating particle acquires the minimum operating energy. Indeed, the uncertainty must be so great that the particle would never acquire it without the influence of the diathete.

And

> . . . it is consistent with everything known in physics to assume that events that cannot be predicted, even in theory, as a result of physical observations are also not determined by the physical forces that act. And it is not inconsistent with anything in physics to assume that such events are determined by influences other than physical forces. . . .

This gives the game away. This is not a criterion for *discovering* a primary relay, but a justification for *assuming* something which is not inconsistent with physics even when physics cannot in principle verify it. The operation of influences other than physical forces may not be inconsistent with anything in physics, but surely physics can throw no light on them and neither can any other science, unless we accept a radically different definition of science from that which seems to be accepted in this book.

In fact, throughout the book Professor Kapp is doing philosophy and would recognise it if he had not a distorted view of the nature of philosophy. True, he admits that it is the task of philosophy to examine the meanings of concepts, and this covers a great deal of what he does; but he regards philosophers, unfairly and all too generally, as speculating without due consideration of the facts discovered by science and as being influenced largely by personal preferences and suggests, I think, that they ask questions without precisely formulating them and are strangers to the discipline of logic. He is hardly kinder to those scientists who have failed to see that the mind-body problem is a problem for science.

I cannot forbear, in conclusion, from saying that not only is Professor Kapp philosophising when he claims not to be, but that he is philosophising badly, so that his arguments will be as unconvincing to those who accept his dualism as to those who reject it.

PETER ALEXANDER

Phénoménologie et Matérialisme Dialectique, Tran-Duc-Thao, Minh-Tân, Paris, 1951. Pp. 368. 650 francs.

This book consists of two parts: the first is a series of studies of the phenomenology of Husserl, which serves as an introduction to the second, an exercise in the very different discipline of dialectical materialism. The whole is claimed to constitute 'la solution positive du problème de la conscience' (dust wrapper). In the first part the author tries to show that a pure analysis of consciousness must force us to realise that all purposive direction originates in the last resort from sensation; and this brings us back 'au contenu matériel de l'existence,' and hence to dialectical materialism. In the second part he begins by giving a general schema of the evolution of 'la vie intentionnelle' from the first sensory-motor patterns in the lower animals to really human modes of adaptation to the world (by the use of symbols); and then goes on to the problems of 'la spiritualité proprement dite.' His conclusion is that 'la revolution prolétarienne, explicitant son contenu positif dans la construction du socialisme et le passage au communisme, est la realisation effective de l'idéal humain aliéné et mystifié dans les religions et les métaphysiques': and we need to get rid of the idealist form of this ideal by bringing out the material dialectic of its content.

Only the first half of the second part is at all likely to appeal to a British public. For British readers, though they are often interested in the psychophysical problem and in the evolution of forms of consciousness, generally are not—and do not propose to become—initiates of either phenomenology or dialectical materialism. For better or for worse both disciplines appear remote, uncongenial and almost unintelligible; like Rabbinical arguments. But if there are, and there should be, any adventurous intellectual travellers who are willing to cross the enormous chasm which now separates philosophy as practised in the United Kingdom from philosophy on the continent of Europe, then this book offers a simultaneous introduction to two different unfamiliar worlds of thought. M. Tran-Duc-Thao has been both enterprising and original in his intellectual bridge building; for previously Marxists have followed Lenin's lead by rejecting holus-bolus Husserl's phenomenology along with the empirio-criticism of Mach; and yet he himself appears to be Stalinist, though unobtrusively so (cf. p. 240 n, where Lysenko is mentioned approvingly: and the dove symbol on the cover). However, though it is good to have built this bridge, what British readers today need most are not bridges between the various continental disciplines but bridges—two-way bridges—between these and the linguistic developments in our own country.[1] But M. Tran-Duc-Thao was not, of course, writing for a British public, and this was not what he was aiming to provide: so it would be unfair to reproach him for not providing it.

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[1] The only criticisms or critical reinterpretations of Marxist philosophy from a 'linguistic' point of view which I know of are by G. A. Paul (*Analysis*, 5, 65), Margaret Macdonald (*Analysis*, 5, 1) and Celia Fremlin (*Analysis*, 6, 10), though

ANTONY FLEW

Foundations of Inference in Natural Science, J. O. Wisdom, Methuen & Co. Ltd., London, 1952. Pp. x + 242. 22s. 6d.

THE theme of this book is that the method of science revolves around the hypothesis and not around induction: not only is induction invalid as a method of inference but it is quite incapable of leading to certain forms of hypothesis and in scientific practice it is not used. The traditional view of scientific logic is completely reversed. What appears to be induction is, in this re-orientated view, assimilated into the method of hypothesis and deduction: the hypothetico-deductive system of Kraft.

The book has three main parts. The first and most important is an account of the hypothetico-deductive approach, the second is a critical examination of induction, and the third discusses attempts to justify induction on a basis of probability. A final chapter attempts to meet the unsolved problems of the second and third parts by means of the author's hypothetico-deductive view of scientific inference.

At the outset the author states that his aim is to examine scientific procedure as it is practised, to study the methods of the 'scientist in action'. It is disappointing therefore to find that, as in most books on scientific method, only a few pages are given to examining actual scientific investigations and that most of the examples cited date from the last century or earlier. After this brief glimpse of scientific practice it comes as less of a surprise than otherwise it would, to read that the most important maxim in experimentation is to vary only one factor at a time—a doctrine long outmoded. R. A. Fisher, in his classical *Design of Experiments*, now in its sixth edition, shows that biometrics often enables one to vary several factors at once with advantage from every point of view. For many years multiple factor experiments have been widely used with success. The brief reference on page 18 to the purpose of 'controls' in an experiment suggest an inadequate appreciation of the basic structure of the experimental method in biology. The purpose of the 'control' group is nothing more nor less than to provide a basis for comparison with the 'test' group; without the control most biological experiments would be almost meaningless.

The author considers that it is important to distinguish between generalisations and 'non-instantial hypotheses'. The former are hypotheses

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there are also some criticisms of certain Marxist political views which exploit the new 'linguistic' techniques (e.g. S. Dominity, *Australasian Journal of Philosophy and Psychology*, May 1948). And Maurice Cornforth has counter-attacked from a Marxist point of view in *Science versus Idealism* and in *In Defence of Philosophy*.

which express generalisations about instances that can be observed. Non-instantial hypotheses are hypotheses that contain at least one concept that is not observable. This latter type of hypothesis can never be arrived at by induction, though, according to the traditional view, the former can. Generalisations can easily be tested. All that is necessary is to examine a large number of instances. But to test a non-instantial hypothesis one must first make deductions from it. The deductions are generalisations and these can be tested by observing instances of them.

The essential features of the scientific method in the author's view are jumping 'by some imaginative means' to a generalisation or a non-instantial hypothesis and then proceeding to test this. Most scientists would agree with this view. Einstein in particular has repeatedly said that original ideas are arrived at by institution and not by any rational method. The author, however, does not take evidence from Einstein or the many other scientists who have written on this matter. Having dismissed induction as the method of arriving at hypotheses, he does not enquire into how they are obtained. Evidently he regards this question as outside the scope of this discussion.

Although there is no *method* of induction, the author concedes, as he must, that a generalisation can be stated as though it were the conclusion of an induction. The main purpose of the book, however, is to show that the inductive form is both useless and incapable of being justified, whereas the hypothetico-deductive framework is more fruitful and more faithfully describes the way scientists work. The author credits Popper with having originated most of the views he expounds in the first part of the book. To further enthrone the hypothesis as ruling all scientific procedure in place of induction, Popper has pointed out that at all stages of an investigation the scientist entertains many hypotheses. These constitute his 'horizon of expectations' without which he would have no basis for selection in making observations; nor could he notice the unexpected if he did not have expectations. Thus the scientist is continually guided by hypotheses which he revises as required and tests by testing deductions made from them.

In Parts II and III Wisdom is on surer ground. This portion of the book contains a thorough critical examination of the views of scientific inference that have been developed over the last thirty years or so, particularly by Keynes and Broad. All attempts to justify induction are found wanting and the positivist view leaves unsolved the problem of justifying an inference from examined to unexamined instances, from past to future. Wisdom believes that the hypothetico-deductive system provides the most satisfactory basis for expecting predictions based on past experience to come true. Our hypotheses are not asserted to be true, but merely as yet unfalsified, and in light of these it is more reasonable to expect certain events in the future than to expect others. This view is realistic and reasonable and many will find it satisfying even though the basic difficulties remain as intractable as ever.

The book merits study by all serious students of scientific logic. The author's main thesis is more in keeping with scientific practice than is the traditional account of the scientific method. The criticism that he has not sufficiently studied scientific research at first hand is one to which, unfortunately, most scientific philosophers are open. The style is pleasing and the reasoning is always clear. There is the usual occasional misprint and on page 18 there is a slip in describing Pasteur's experiment, which concerned anthrax, not rabies as is stated. The book is attractively produced.

W. I. B. Beveridge

Leçons de Logique Algébrique, Haskell B. Curry, *Collection de Logique Mathématique*, Série A, No 2, Gauthier-Villars, Paris and Louvain, 1952. Pp. 164. Fr. 1,600.

During the academic year 1950–51 Professor Curry gave a course of lectures in the University of Louvain. This book is based on these lectures, which were primarily addressed to philosophical logicians and to mathematicians without a technical knowledge of logic. It only presupposes the 'ability to think abstractedly and to reason with symbols'. Professor Curry regards philosophical logic as the science which investigates valid inferences, whilst the mathematical systems which result when mathematical methods are applied to philosophical logic he calls mathematical logic. As a third use of the word 'logic' he mentions the various particular systems of logic such as two-valued logic, intuitionist logic, etc.

In Chapter I the notion of formal system is defined and illustrated. On page 27 the author writes:

> On peut adopter la notion de système formal comme idée centrale de la mathématique. La mathématique peut être définie comme la science des systèmes formels. La logique mathématique est donc l'étude de ces systèmes formels qui ont vis-à-vis de la logique philosophique le même rapport que la géométrie vis-à-vis de l'espace.

Chapter II is devoted to distinguishing the varieties of logical algebras, i.e. systems 'avec des variables libres (mais sans variables liées), et une relation fondamentale transitive'. This chapter concludes with a list of interpretations of logical algebras. Chapter III deals with those systems which are called lattices and which were defined in the preceding chapter. Various special kinds of lattices are defined and theorems concerning them are proved. Chapter IV, which occupies thirty pages is devoted to the theory of implication, and a fifth chapter of the same length deals with various kinds of negation. The book ends with a short chapter entitled *Les algèbres* *plus compliquées*, and an Appendix which deals chiefly with the bracket-less notation of Łukasiewicz. There is an extensive bibliography and an index.

It is most useful to have all this material so skilfully brought together in one book and so clearly presented with the help of a uniform method. There appears to be an error on p. 40, second line, where the horizontal arrow should point to the right.

J. H. Woodger

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

W. Ross Ashby, *Design for a Brain*, Chapman and Hall, London, 1952, pp. ix + 259, 36s.

Sir Edward Bailey, *Geological Survey of Gt. Britain*, Allen & Unwin, Ltd., London, 1952, pp. xii + 278, 22s. 6d.

Stewart C. Easton, *Roger Bacon and his Search for a Universal Science*, Columbia University Press, New York, 1952 (Blackwell, Oxford), vii + 255, 25s.

Paul Haberlin, *Philosophia perennis*, Springer-Verlag, Berlin, 1952, pp. 161, DM. 12.60 (Cloth, DM. 16.80).

A. R. Hall, *Ballistics in the Seventeenth Century*, Cambridge University Press, London, 1952, pp. viii + 186, 21s.

R. M. Hare, *The Language of Morals*, Clarendon Press, Oxford (Geoffrey Cumberlege), 1952, pp. vii + 202, 7s. 6d.

Leonard Linsky, *Semantics and the Philosophy of Language*, University of Illinois Press, Urbana, Ill., 1952, pp. ix + 289, $3.75.

Anneliese Maier, *An der Grenze von Scholastik und Naturwissenschaft*, Edizioni di Storia e Letteratura, Roma, 1952, pp. vii + 387.

G. Van Melsen, *From Atomos to Atom, The History of the Concept Atom*, Duquesne Studies, Philosophical Series 1, Duquesne University Press, Pittsburgh, Pa., published in Britain by Basil Blackwell, Oxford, 1952, translated by Henry J. Koren, pp. xii + 240, 32s. 6d.

J. B. Rosser and A. R. Turquette, *Many-valued Logics* (Studies in Logic and the Foundations of Mathematics), North-Holland Publishing Company, Amsterdam, 1952, pp. viii + 124, 12 guilders.

Bertrand Russell, *The Impact of Science on Society*, Allen & Unwin, Ltd., London, 1952, pp. 140, 7s. 6d.

George Sarton, *A Guide to the History of Science*, Chronica Botanica Co., Waltham, Mass. (London : Wm. Dawson & Sons Ltd.), 1952, pp. xviii + 316, cloth $7.50.

Baruch Siman, *Der logische Positivismus und das Existenzproblem der höheren Ideen*, Herausgegeben von der Antikommunistischen Liga in Tel-Aviv, pp. 160.

P. F. Strawson, *Introduction to Logical Theory*, Methuen & Co. Ltd., London, 1952, pp. x + 266, 15s.

Symposia for the Society for Experimental Biology No. VI, Structural Aspects of Cell Physiology, Cambridge University Press, London, 1952, pp. 356, 45s.

Lynn Thorndike, *The Sphere of Sacrobosco and its Commentators*, University of Chicago Press (London : Cambridge University Press), 1949, pp. x + 496, 75s.

Webster Prentis True, *Panorama of Science*, The Series Publishers Inc., New York, 1952, pp. ix + 422.

Edited by G. M. Wickens, *Avicenna—Scientist and Philosopher*, Luzac & Co., London, 1952, pp. 128, 15s.

A. Wolf, *A History of Science, Technology and Philosophy in the XVIII Century*, first published in 1938, second edition revised by D. McKie, George Allen & Unwin, London, 1952, pp. 814, 60s.

J. H. Woodger, *Biology and Language* (Tarner Lectures, 1949-1950), Cambridge University Press, 1952, pp. xiii + 364, 40s.

(*b*) JOURNALS

The contents of the latest issues of MIND *and of the* JOURNAL OF THE HISTORY OF IDEAS *are printed with the advertisements on the cover of this issue*

Dialectica (Zurich, Switzerland), 1952, **6**, No. 1
- F. Gonseth, ' Motivation et structure d'une philosophie ouverte '
- G. Isaye, ' Le privilège de la métaphysique '
- F. Gonseth, ' Réponse au R. P. Isaye '
- M. F. Sciacca, ' Autonomie de la métaphysique et " intériorité objective " '
- R. P. Sottiaux, ' Lettre à M. Gonseth sur la fidélité inébranlable '
- Ch. Perelman, ' Réponse a M. Bernays '

The Journal of Symbolic Logic (Baltimore, U.S.A.), September, 1952, **17**, No. 3
- J. Kalicki, ' A test for the equality of truth-tables '
- J. Myhill, ' A finitary metalanguage for extended basic logic '
- A. Church and W. V. Quine, ' Some theorems on definability and decidability '
- W. Craig and W. V. Quine, ' On reduction to a symmetric relation '
- N. Goodman, ' New notes on simplicity '
- J. Myhill, ' A derivation of number theory from ancestral theory '

Methodos (Milan, Italy), 1952, **4**, No. 13
- G. Vaccarino, ' Consapevolizzazione del formalismo ' (with English translation)
- Béla von Juhos, ' Die " Wahrheit " wissenschaftliche Sätze und die Methoden ihrer Bestimmung '
- D. Faggiani, ' Sulle propozioni primitive della Fisica ' (with English translation)
- J. Piaget, ' La logistique axiomatique ou " pure ", la logistique operatoire ou psychologique et les réalités aux quelles elles correspondent '
- No. 14, S. Issmann, ' Problèmes de la définition '
- L. A. Lezama, ' La unidad de la filosofia y de la ciencia en una metodologia universal ' (with English translation)
- C. K. Davenport, ' The role of graphical methods in the history of logic '
- P. F. Linke, ' Eigentliche und Uneigentliche Logik '

Philosophy of Science, July 1952, **19**, No. 3
- D. W. Gotshalk, ' Value science '
- H. Margenau, ' Physical versus historical reality '
- W. H. Werkmeister, ' The problem of physical reality '
- C. L. Golightly, ' Mind-body, causation and correlation '
- E. H. Madden, ' The philosophy of science in Gestalt Theory '
- J. P. McKinney, ' Discussion '

Theoria, 1952, **18**, Parts 1-2
- S. Halldén, ' Kants Kritik des ontologischen Gottesbeweises '
- H. Johansen, ' Die Russellsche Theorie der definiten Deskriptionen vom Standpunkt der Sprachwissenschaft aus betrachtet '

(*c*) ARTICLES

Wilfred Barlow, ' Postural Homœostasis ', *Annals of Physical Medicine*, 1952, **1**, 77-87

G. A. Birks, ' The ultimate synthesis—physical or psychological ', *The Philosophical Quarterly*, 1952, **2**, 307-22

Mario Bunge, ' La epistemología positivista ', *Nosotros*, 1943, **8**, 283
- ' El Tricentenario de Newton ' (booklet, 1943)
- ' Significado físico e histórico de la teoría de Maxwell ' (booklet, 1943)
- ' ¿ Que es la epistemología ? ' *Minerva*, 1944, **1**, 27
- ' Auge y fracaso de la filosofía de la naturaleza ', *Minerva*, 1944, **1**, 213
- ' Nietzsche y la ciencia ', *Minerva*, 1944, **2**, 44
- ' La fenomenología y la ciencia ', *Cuadernos Americanos* (Mexico), 1951, **10**, No. 4, 108
- ' The inexhaustible electron ', *Science and Society*, 1950, **14**, 115
- ' Mach y la teoría atómica ', *Boletín del Químico Peruano* (Lima), 1951, **3**, No. **16**, 12
- ' What is chance ', *Science and Society*, 1951, **15**, 209

Arthur W. Burks, ' Reichenbach's theory of probability and induction ', *The Review of Metaphysics*, 1951, **4**, 377-93
- ' The logic of causal propositions ', *Mind*, 1951, **60**, 363-82

Adolf Grünbaum, ' Relativity and the atomicity of becoming ', *The Review of Metaphysics*, 1950, **4**, No. 2
- ' Some recent writings in the philosophy of mathematics ', *The Review of Metaphysics*, 1951, **2**, No. 2
- ' Some highlights of modern cosmology and cosmogony ', *The Review of Metaphysics*, 1952, **5**, No. 3.
- ' Messrs. Black and Taylor on temporal paradoxes ', *Analysis*, 1952, **12**, No. 6

W. K. C. Guthrie, ' The presocratic world-picture ', *The Harvard Theological Review*, 1952, **45**, No. 2

G. Kreisel, ' Note on arithmetic models for consistent formulae of the predicate calculus ', *Fundamenta Mathematicae*, 1950, **37**, 265-85

Heinz Niehrs, ' Die Struktur grundlegender Relationen zwischen den Werten einer " Grösse " ', *Philosophia Naturalis*, 1952, **1**, 493-510

MEETINGS OF THE PHILOSOPHY OF SCIENCE GROUP

The following meetings were held during the academic year 1951-1952. They took place at 5.30 p.m. in the Joint Staff Common Room, University College, Gower Street, London, W.C.1.

Monday, 15 October 1951 : Mr W. Kneale on 'Observables and Unobservables'
Monday, 12 November 1951 : Professor W. I. B. Beveridge on 'What is wrong with the Philosophers' Accounts of Scientific Method ?'
Monday, 10 December 1951 : Professor R. O. Kapp, on 'Problems and Theories of Causation'
Monday, 21 January 1952 : Professor R. W. Russell on 'Problems of Method in Psychology'
Monday, 18 February 1952 : Dr R. S. Silver on 'The Methodology of Applied Science'
Monday, 3 March 1952 : Annual General Meeting
Monday, 28 April 1952 : The Chairman (Professor K. R. Popper) gave an address on 'Scientific Origins of some Philosophical Problems'
Monday, 9 June 1952 : Professor M. Polyani on 'The Nature of Beliefs'

OBITUARY

SIR CHARLES SHERRINGTON, O.M., F.R.S., 1857-1952

The death of Sir Charles Sherrington on 4th March 1952 brought to a close at the age of 94 the life of a great scientist and scholar. But he lives on in his writings and in the inspiration that he has given his pupils. For neurophysiologists he has been described as ' the man of genius who laid the foundations of our knowledge of the functioning of the brain and spinal cord '. But in addition his pupils have regarded him with love and reverence both as a father and a friend, and they feel bound together by personal ties as if they belonged to one family. The readers of this *Journal* will also have a special interest in him because he was a member of the Editorial Board, and the writings of his later years have earned him the title of ' The Philosopher of the Nervous System '.

Sir Charles Sherrington was one of the very select group of men who grow in scientific stature throughout their lives. It is safe to predict that for Sherrington this growth will still continue posthumously. For it is now nearly twenty years since his last original scientific contributions, and yet his concepts stand as the foundations on which are built the most recent investigations on the physiology of the central nervous system. To these select few it is given to bring order into confused fields of scientific investigation, to find the right and fruitful paths amidst the tangled ideologies that flourish at the outset of a new science.

When we look at the first years of Sherrington's scientific career there is little to suggest his great destiny. Though he started with some neurohistological work, there was a period of several years when his scientific interests lay in such varied fields as the pathology of cholera, haematology and general pathology. But gradually his attention concentrated on neuro-anatomy, and neuro-anatomy led on to neurophysiology.

At the close of his scientific life he looked back on this period of neuro-anatomy as essentially one of preparation for the neurophysiology that was to come. Necessarily, of course, neurophysiology of the central nervous system can be investigated only when a framework of neuro-anatomy has been securely established. Sherrington was fortunate at the outset because his European visits made him familiar with the magnificent work of Cajal and Golgi on the microscopic structure of the central nervous system. But his own meticulous investigations on the segmental mapping of the motor and sensory functions of the spinal cord and on the sensory fibres of muscle nerves were necessary preliminaries to his own great field of investigation in the reflex functions of the spinal cord, for that formed the centre of his attention for nearly forty fruitful years. The fruits of those years are the foundations of modern neurology. There were of course interludes. Such an active mind would be attracted at times, by alluring prospects of investigation, into studies on electrical stimulation of the motor cortex and on binocular vision and visual contrast phenomena. But always he returned with fresh insight to the reflex functioning of the spinal cord and particularly to the role of spinal reflex mechanisms in the control of muscular movement.

It is remarkable how surely he discovered the way so that there was a steady progress in understanding with almost no occasion for retracing of steps. When we examine his work in an attempt to discover the secret of his amazing success, we can find several clues.

First, meticulous study of structure precedes and goes on simultaneously with the functional study. Sherrington's concepts were always formulated in terms of known structures and he showed great insight in grasping the essential structural features in the vast maze described by neurohistologists. For example it is to him that we owe the name and the physiological concept of the synapse.

Secondly his investigations were not ends in themselves, but always it seems that he had before him the great problem : How does the nervous system work not only in isolated units but also in its functional entirety ? This gave a meaning and a direction to his work as well as guidance in the choice of research problems. One can survey his life's effort as the ordered strategy of a campaign.

Again he seemed to have an intuitive grasp of the essential functional features of the nervous system and he coined such appropriate terms for them as Principle of Convergence, Final Common Path, Reciprocal Innervation, Motor Unit.

Added to all this he had a mastery of the technique of mammalian experiment. He was exceptionally skilled as an operator, handling living tissues with a delicacy that was rare in those days, and he was an acute observer. The technical equipment of his laboratory would look modest in comparison with the electronic gear in a modern neurophysiological laboratory, but the precise analysis of reflex muscular contraction that characterised his Oxford school was made possible by the isometric optical myograph that he developed, and the string galvanometer was also used extensively in those pre-electronic days.

It is possible to survey Sherrington's achievement also by the three books that each mark a stage of development of his study of the nervous system.

The Integrative Action of the Nervous System, published in 1906, has rightly become the classical book of neurophysiology and was republished in 1947 on the occasion of the International Physiological Congress at Oxford.

It gives essentially an account of the way in which the nervous system comes to react reflexly in a meaningful way to diverse sensory inputs: the flexor reflex withdrawing the limb from the harmful stimulus; the scratch reflex removing the skin irritation; the walking reflex giving progression and so on. That account still introduces the student to the study of the central nervous system, and forms the basis of our further investigation.

The *Reflex Activity of the Spinal Cord,* published in 1932, represents the more precise and analytical investigation of Sherrington in his Oxford school. Various reflexes are studied in detail in order to discover the way in which the individual nerve cells react and even to throw light on the excitory and inhibitory actions of the synapses. But integration was not neglected and the concept of the motor unit enabled Sherrington in the last chapter to write a masterly account of the reflex control of muscular movement.

Finally, in 1941, in *Man on his Nature* Sherrington stated his philosophy as a neurophysiologist; a second edition was published in 1951. Essentially this was an attempt to show that an understanding of the structure and function of the living body did not give a complete explanation of man. For example, he states that the mental and the matter-energy entities ' for all I can do remain refractorily apart. They seem to me disparate; not mutually convertible; untranslatable the one into the other '. Yet the mystery is that they do interact. Thus his philosophical position is that of frank dualism and interactionism. During the last twenty years of his life these philosophical problems came more and more to dominate his thought, largely replacing his life-long devotion to neurophysiology. A few days before his death he said that he realised that this philosophical work of his had fallen on an unfavourable climate of opinion, but he did not regret it. The more he pondered on the mystery of man, the more sure he became of the essential duality of man's nature. Man is both matter and mind, and minds amount to ' all that counts in life '. Just as the *Integrative Action* has modelled our thinking and research on the central nervous system for nearly fifty years, so it may be predicted that *Man on his Nature* will reorientate scientific thinking on man himself. It would seem too that Sherrington over-estimated the opposition of his scientific colleagues to his philosophical ideas. Certainly he has been heavily condemned in the U.S.S.R., as may be seen in the recent Pavlov Report, but such condemnation is becoming a mark of distinction outside the Iron Curtain. In the free world the opposition would be largely restricted to the dogmatic materialists.

It is difficult to write briefly about the personal qualities of ' Sherrie ' as he was known to all of us: the modesty, the charm, the vivacity, the good humour, the wonderful range of knowledge on which he could draw with singular aptness. It was an amazing experience to live in daily intimate contact with him. And one always associates him with the informality and warmth of the hospitality that Lady Sherrington gave to their home in Oxford. All these remain the rare memories that we treasure throughout our life time, memories that become more precious as we learn that such occasions only come once. Sherrington was a scholar and a scientist, an adventurer questing for truth and beauty, and so human a person that his pupils' admiration was transmuted into love. It is not to be wondered that, a poet himself, he was most deeply attracted by Keats.

J. C. Eccles